THE LANCET.

VOL. I.] LONDON, SATURDAY, DECEMBER 29, 1838. [1838-39.

COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XIII.

Capillary circulation.—Ideas respecting the spontaneous movement of the globules refuted.—Proofs that the capillary circulation depends on the heart's motion.—Coagulation at a low temperature.—Effects of loss of coagulability.—Transfusion of blood.—Action of carbonate of soda.

GENTLEMEN:—I shall, in this lecture, proceed with the subject of the capillary circulation. I may premise by informing you that the movement of the blood in the capillaries of one's own body may be seen in the following manner:—Place yourselves in a strong light, close your eyes, and then stretch the upper lids so as to render them as thin as possible; they will permit the passage of a few luminous rays, and you will perceive, though of course indistinctly, the blood moving from the upper part of the lid towards the tarsal cartilage. In investigating the capillary circulation in animals the microscope is always employed, and the animals on whom such examinations may be most effectually made are frogs and salamanders, among batrachians; mice and small rats, among mammalia. The tail of some fishes, too, is, from the transparence of the integuments, well adapted for inspections of this kind. I shall extract the greater part of the facts I am about to lay before you from M. Poiseuille's "Essay on the Capillary Circulation." As this Essay has not yet been printed, I shall quote some passages verbatim, commencing with the simplest phenomena.

The experimentalist separates the femoral artery and vein from their connections with the surrounding tissues by delicate dissection, and then passes a ligature round the thigh, taking care to tighten it forcibly; the circulation of the part is now carried on solely by the two vessels named. The animal is pinned down to a plate of cork, so as to make the interdigital spaces correspond to the object-glass of the microscope. When he has ascertained the degree of velocity of the globules he intercepts the course of the blood in the artery, leaving the vein free. The globules still continue to move, but they do so more slowly; and their movement gradually grows slower and slower, until, at the end of two or three minutes, it ceases completely. When the compression is taken off, each globule which was in a complete state of rest, instantly starts off with the rapidity of an arrow and recovers its normal velocity. Some physiologists, on observing the persistence of the movement of the globules after the impulsion of the heart had been prevented from acting on them, were, very naturally perhaps, induced to recognise a sort of inherent progressive force in those bodies, which, they further supposed, directed them from the arteries towards the veins. Others conceived that the latter vessels exercised a kind of aspiration on the globules. Both these notions are erroneous. The same phenomenon to which I have already drawn your attention, as existing in the vascular trunks, occurs in the capillary vessels. The facts with which you are already well acquainted show that the movement of the globules is to be explained by the elastic retraction of the arterial walls below the ligature. When the passage of the blood is intercepted in a large artery the vessel retracts abruptly, the diminution of its diameter takes place suddenly. In the capillary system, on the contrary, retraction is a slower process, and this difference in point of elasticity in the small and large tubes, explains why, in the experiment I have just described, the globules continue to move for several minutes after the application of the ligature.

The mesentery of a frog is next separated from the animal, and spread out on a piece of glass. A certain quantity of blood escapes from the opened vessels, and as those are no longer dilated by the column of blood impelled forwards by the heart, they re-

tract, and their retraction is so considerable that the diameter of some veins and arteries decreases to half its original amount. The flow of blood only ceases when the vessels have reached the utmost limit of elastic retraction; and the greater number do not undergo retraction throughout their entire extent, there are swollen points observed here and there. Now, this irregularity in the retraction of the capillary walls depends on the manner in which the small quantity of blood remaining in each tube is distributed and undergoes coagulation. Masses of globules accumulate in several points, and to each of these accumulations corresponds a swelling of the vessel; for the coats being arrested in their retraction by a physical obstacle are unable to retract so fully as if the cavity were empty. The notion of the existence of different degrees in the retractile force of the capillaries is, therefore, an incorrect one: remove any clots they may contain, from their interior, and the diminution in their calibre will be the same through their entire extent.

One of the chief proofs adduced in support of the opinion that the globules are endowed with a faculty of spontaneous movement, is derived from the following experiment. It has been said that if, while you examine the circulation in a capillary vessel, you make a small hole in any point of its walls, the direction of the current within it is instantly modified. The whole column of blood, which a moment before obeyed one and the same impelling power, separates into two distinct columns: these, moving in contrary directions, rush towards the little orifice you have made, and effect their escape through it. The action of the heart has ceased to be felt beyond the accidental opening,—the movement of the globules there situate cannot, therefore, be explained by that action; besides their movement is a retrograde one. What other cause, it is asked, but an act of their will, can cause these intelligent corpuscula to move in this novel direction? The fact, Gentlemen, is unquestionably correct, and the inference drawn from it is, no doubt, specious enough; but a little reflection clearly shows it to be faulty. We know that the pressure exercised by the blood in the normal state on the walls of the arteries and veins is superior to that of the atmosphere. Now, this pressure is suddenly diminished at the point of section; to this point, consequently, a rush of blood must, in pursuance of the laws of the equilibrium of bodies, take place. Whatever be the existing direction of their course it will now become that of the artificial opening: this movement of the globules is also promoted by the elastic retraction of the walls of the vessels, which press circularly on the blood, and, in the absence of the impelling influence of the ventricle, communicate a retrograde motion to it. I see no necessity for imagining hypotheses, when the physical explanation of the phenomenon is so easy and natural. But, further, if you distend a caoutchouc tube with an injection, and then make an opening in its central part, the contained liquid will rush thither from both ends of the tube. The case is the same as with the living vessel.

The oscillations of the globules, their times of stoppage, and their movements in various directions, depend, so long as the coats of the tubes are uninjured, on the position they hold with respect to the motionless stratum. A spontaneous obstruction frequently takes place through an accumulation of globules: those which happen to be in the neighbourhood of a collateral branch escape by it; others advance and retreat while waiting for the passage to become free again. Once the obstacle is passed they all resume their ordinary speed. When a capillary vessel contains a greater number of globules than its neighbour, the velocity of the current within it decreases greatly; this is the direct consequence of the contact of the globules with the peripheric layer of fluid. Let me next suppose an experiment in which you have separated a frog's web from the rest of the animal; the globules continue to move for a certain time; in the arteries the blood retrogrades; in the veins, on the contrary, it pursues its natural course. After a certain lapse of time the vascular tunics regain their normal diameter by help of their elasticity, and all is quiet in their interior. If you now slope the object-glass slightly the globules immediately move towards the dependant part, if the blood be still uncoagulated; but this change of place is not the result of a power inherent in them; it is a simply physical effect of their specific gravity being greater than that of the serum in which they float. If you slope the object-glass in the opposite direction, the globules change their position accordingly; place it horizontally, and you will find that the equilibrium becomes gradually established, and that, after a few oscillations, the globules again become motionless.

Hear another experiment, which proves in the clearest manner that the movement of the blood in the capillary vessels depends on the impulsion of the heart and the elastic retraction of the walls of the vessels. I extract it verbatim from M. Poiseuille's manuscript:—"The femoral vein, artery, and nerve of a frog are accurately separated to the extent of two centimetres, at the least, from the surrounding tissues, and a ligature then passed round the thigh, excluding the vessels and nerve; a loose ligature, ready to be tightened at will, is thrown round the vein. A thread is next fixed to the extremity of each digit of the same limb, to facilitate the examination of the circulation in the interdigital spaces, without modifying it

by pricking the tissues. The frog being pinned down to a flat piece of cork, and the web laid under the object-glass of the microscope, the ligature embracing the bone and muscles of the thigh is forcibly tightened. The experimenter is then certain that the circulation in the lower part of the limb is carried on by the dissected vessels alone. The circulation in the arteries, veins, and capillaries, goes forward in the same manner as before the performance of the operation described; jerking movements sometimes take place. The globules move more rapidly in the arteries than in the veins, and in the capillaries their velocity is less than in the other two orders of vessels; in some, however, it is greater than in others, for reasons to which we need not at present direct our attention. The observer now watches with especial care an artery and vein of the interdigital space submitted to investigation; he then interrupts the course of the blood in the femoral vein: the moment he does so the progression of the globules in the vessels of the digital interspace under examination becomes jerking, and this jerking mode of progression lasts a few seconds only, being followed by an oscillatory movement. The span of these oscillations at first equals the length of five globules, and soon decreases to that of two; the rhythm is identically the same in the artery and capillaries of the interdigital space, and they continue, to the number of forty-six in a minute, so long as the compression of the vein is kept up. While the femoral vein still undergoes compression, the experiment is varied by interrupting the course of the blood in the artery also; the oscillatory motion ceases at the same instant. The globules become quiescent in the artery, the capillaries, and vein of the extremity. If the femoral artery be then freed from constriction, oscillations of equal length in the three orders of vessels recommence. These experiments concluded, the heart of the animal is laid bare, and the number of the contractions of the ventricles counted; these are found to be one hundred and eighty-six in four minutes, or forty-six in a minute.

"*Remarks.*—The oscillations of the globules are produced, on the one hand, by the heart, which impels the blood into the arterial system, into the capillaries and the veins; on the other, by the retraction of the arteries and veins that follows their dilatation by the wave of blood driven forwards by the left ventricle."

One of the great objections urged against the opinion of those who deny the spontaneous movement of the globules is this: if you separate a portion of a living animal from the rest of its frame, as, for example, the web of a frog, you may see that the blood continues to move in the vessels, and escape by their open orifices. Here, then, it is urged, there is a force in action on the globules independent of that of the heart. I cannot admit the correctness of this assertion; the movements that are observed in the globules arise simply from the flowing of the liquid, which suffers a less degree of pressure where the vessels are gaping than elsewhere. It is exactly the same thing as if I distended an elastic tube with an injection: when both its extremities are closed the liquid is quiescent, but the force communicated to the latter by the syringe still exists, though dormant, and becomes apparent the moment the cause producing the temporary equilibrium ceases to act. Turn the two cocks, and the liquid will escape by both points of issue: the same phenomenon occurs in the capillary vessels; their walls retreat in virtue of their elasticity. In the first case the hydrodynamic power is a syringe; in the second a muscular pump. Why, then, call by a different name the effects, identical in nature, which take place in both? Can what is physical in essence become vital just as you will it? I am well aware that of the two agents, which have each of them dilated a tube, one is organised, the other inorganic, but the dilatation which results from their action is an essentially mechanical phenomenon. For these reasons the experiment adduced by our adversaries in support of the opinion they espouse really militates against its correctness. We will pursue this subject when we next meet.

I told you, Gentlemen, that physiologists had, in general, come to no important or satisfactory results respecting the coagulation of the blood; and you will grant readily that it could not be otherwise, when you reflect on the unimportant character of the question about which they have busied themselves. Thus, for example, they have laboured to ascertain whether disengagement of caloric accompanies coagulation. If we call to mind the physical laws that preside over the changes of form assumed by matter, it seems as though we have in them an *à priori* solution of the problem. We know that when any body passes from the liquid to the solid form its molecules approximate, its volume decreases, caloric and electricity are disengaged, and elevation of temperature consequently ensues; but this application of physical laws to the blood has not been accredited by experience, and is, therefore, inadmissible. The way to avail ourselves most usefully of applications of the kind to physiology is not to abuse them; in other words, to acknowledge the justness of those only which are confirmed by direct experiment.

Another question that has been much agitated is, whether coagulation can take place at a temperature below zero. I submitted some blood to a temperature of -14°, but found that before congelation could take place an exceedingly firm and consistent coagulum had formed. I sought, by another

experiment, to establish the influence of high temperature; the result was, that blood exposed to a heat of from 50° to 55° Réaumur, coagulated exactly as in the ordinary atmospheric medium. From the experiments I have myself made it consequently follows that neither *cold*, *heat*, *rest*, nor *motion*, prevent the occurrence of coagulation; however, we are acquainted with one condition in which it will not take place, whilst the blood is kept in motion; this is when we receive arterial blood directly from an artery into the body of a syringe which has been heated to 30° Réaumur; so long as the liquid in the inorganic instrument receives the impulsion communicated by the heart it does not form into a mass. You recollect how I availed myself of this circumstance in practising the transfusion of the blood of one animal into the vessels of another: it is not necessary that the quantity of fluid be small; we may collect nearly half a litre in a syringe, and the portion of it farthest from the left ventricle will not coagulate sooner than that which is nearest. But be all this as it may, you observe, Gentlemen, that these questions only possess such a share of interest as we may please to accord them; all the importance of the subject merges in the question of the coagulation, properly so called, of the fibrine. You are aware that I have been enabled to neutralise this property by the agency of various substances; and you also know that when the blood is deprived of its coagulable element it can no longer set into a mass; witness the blood in this vessel, which has already lain in it two days, and which you perceive, on my shaking the vessel in the least, is still perfectly fluid. But these are not the only agencies that modify coagulation; there are some substances which produce the same effect by inoculation, such as the virus of rabies, the poison of the viper, and many others, which it is unnecessary to enumerate, especially as I trust, at a later stage of the course, to be enabled to devote a few lectures to these high questions of pathology.

The fact that gives its chief importance to the study of coagulation is that, as has been shown to demonstration, the loss of this property by the blood, no matter under what circumstances it occurs, is followed by death. You remember that I refuted, by anticipation, an objection which might have been made to this statement. The question it regards is, whether the action of a substance on the blood is the same when injected into the vascular system, and when simply placed in contact with that fluid in a vase; in other words, whether vitality does or does not prevent chemical reaction. The investigation I have made leaves no doubt on the subject: *it is undeniable that the substances which you see liquefy the blood in glass vessels, act in the same manner on that fluid in the living tubes of our organs.* Preoccupied as men have been by the opposite opinion, they have taken no notice of the truths I have brought to light; nay, more, you shall learn, by a single fact I am about to relate, how deeply erroneous this prevalent opinion is, and what serious mischief must have been caused by ignorance of the history of the blood.

A celebrated surgeon, M. Dieffenbach, of Berlin, undertook to reinstate, in the favour of medical men, the operation of transfusion of blood from one individual to another, recommending it more especially in cases of obstinate bleeding from wounds, and in uterine hæmorrhage. Now, as he had remarked that the coagulation of the liquid interfered with the success of the operation, he modified the mode of performing it, by extracting the fibrine of the blood he was about to inject, for the purpose of preventing the formation of clots, and the obstruction of the capillary vessels. Experience (that of M. Bischoff, of Heidelburg, among others) has since shown what melancholy results must be the necessary consequence of such a method of proceeding; I need scarcely warn you against ever having recourse to it. You have witnessed the results of the injection of defibrinised blood into animals in good health; what terrible effects may we not justly expect it to produce in individuals whose organism is already, at the time of operation, suffering under some morbid influence? However, transfusion, properly performed, is by no means to be disdained as a therapeutic measure; I have no doubt, indeed, that under certain circumstances, it may be employed with advantage, especially if the blood be procured by adapting the point of a syringe to an artery, and allowing the instrument to fill by means of the unassisted pressure of the liquid. The contractions of the heart cause the ascent of the piston, and the syringe soon fills with perfectly fluid blood, which may be injected without apprehension of mischief. Since I have employed this plan you have never known a fatal issue in cases where I have performed the operation of transfusion.

It has just this moment struck me, that we might modify the process of transfusion with advantage in this way: I will take a defibrinised animal, and at the same moment as an assistant transfuses the blood of a healthy animal into its veins, open an artery, so that in proportion as the normal blood flows in the defibrinised liquid will flow out, and mingle as little as possible with the other. If the experiment prove successful, the plan will henceforth serve for replacing blood of deleterious qualities by such as is fit to carry on the functions that constitute life. This species of transfusion would not be more difficult or dangerous in execution in the case of the

human subject, than that which we have so frequently practised on animals. There are arteries in the human body easily laid bare, and from their juxta-position to osseous surfaces, as the temporal, for instance, effectually compressible, if we do not wish to have recourse to ligature.

In support of what I just stated respecting the effects of defibrination, you may examine the body of this animal, from which I removed successive portions of blood, reinjecting them into the veins, after having first deprived them of their fibrine. Symptoms of great gravity came on almost immediately, and the animal soon perished. Its blood had become so utterly unfit for circulation in the capillaries of its organs that it was extravasated into the various tissues, but especially into the parenchyma of the lung, to which it has given the appearance of a huge clot. With facts like these staring them in the face can men persist in contesting the permeability of our membranes, and the real existence of certain properties in the blood, whereby alone it is enabled to traverse the innumerable vascular canals of our tissues without undergoing exbibition? Again, if instead of at once removing all the fibrine from the blood of an animal we remove it by small portions, we find that these repeated subtractions induce local lesions, of which there is no mistaking the origin. While this is going on a very curious phenomenon takes place; the fibrine of the blood instead of diminishing, actually augments in volume. I offered you a conjectural explanation of this strange fact last year, which at least accounted perfectly well for the sudden emaciation of animals whose blood is defibrinised; I have learned nothing new on the subject since then, so that my original hypothesis is neither overturned nor confirmed.

In order to allow you to judge of the fact for yourselves, I have caused three bleedings to be practised, at equal intervals, in a large dog; one of twelve ounces, the second of ten, the third of eight ounces. On each occasion the blood was beaten up, filtered through a linen cloth, and then immediately reinjected into the veins of the animal: here, in these three vessels, is the fibrine furnished by the blood of the three bleedings. The first is whitish, supple, and elastic, in fact normal in its characters; the second is softer, more spongy, and more voluminous, though the quantity furnishing it was less; these characters are still more distinctly marked in the fibrine of the third bleeding; it is much more voluminous than in the previous instance; its fracture, instead of being clean, shows irregular filaments, proving that the force of cohesion, by which it resisted traction, is unequally shared by its fibres. The normal fibrine has, therefore, undergone, by venesection and the reintroduction of defibrinised blood into the circulation, very remarkable modifications in its physical properties, in its volume, elasticity, and weight. When the two are weighed comparatively, it is found that the healthy fibrine, though inferior in point of mass, weighs considerably more than the other. Among the chemical characters of the two substances there is one which renders it impossible to confound this fibrine of secondary formation with the normal species; it is that if the former be submitted to a temperature of 60° centigr. in a sand-bath, it liquefies just like so much albumen. This is the substance, Gentlemen, to which I have given the name of *pseudo-fibrine*. It is important to recollect that the animals on which these experiments were made, were invariably fed with the feculæ which contain no fibrine. The fibrine must, therefore, I still think, as I did last year, be got from the organs themselves, and thence reconveyed into the circulation; the muscles become atrophous, and wherever fibrine existed a notable diminution in the volume and aspect of the parts ensues.

It follows from the facts I have detailed that it is possible, by gradually defibrinising the blood, to alter its normal properties without, of necessity, depriving it of the power of coagulating. Henceforth, therefore, it will not be enough to examine if such and such blood contain fibrine, and in what quantity it contains it; we must also satisfy ourselves of the nature of that fibrine. This easy analysis of the blood will, I trust, be habitually had recourse to; by it we are enabled to obtain, without difficulty, some certain knowledge of the constitution of individuals; whereas the superficial examination now resorted to by medical men is calculated to lead them into error.

We have already remarked that normal blood, placed in a glass vessel with certain substances, coagulates, but that when mixed with others no solidification takes place. To pursue this view of the subject of coagulation, let me show you some new experiments I have been making. Here is some blood which was, immediately on its removal from the artery, mixed with putrefied water; there is not the least trace of coagulation in it. In this other vessel ordinary water and blood have been mixed together; the whole has set into a mass; it is evident that in the first instance it is not the water that prevented the blood from clotting, inasmuch as water, unmixed with putrid matters, has no effect on the coagulation of the same blood; the liquefaction must, therefore, be ascribed to the putrid particles held in suspension in the water, and, without doubt, to the hydrosulphate of ammonia formed by the process of putrefaction. This is really an important fact. In investigating it further I was first desirous of ascertaining whether a considerable quantity of the putrid liquid was necessary for the

production of serious disorder, and I learned that a few drops only injected into the veins of an animal kill it almost instantaneously. The importance of this subject cannot be questioned, for the human being is constantly exposed, through respiration and otherwise, to the chance of the introduction of such deleterious molecules into his blood; once received into the circulation they soon manifest their presence by disorganising the liquid with which they are mingled. If you examine the blood of an individual who has succumbed under the terrible symptoms developed by the absorption of miasmata, you will find that in point of colour, fœtidness, and liquidity, it accurately resembles the blood in this vessel; here, then, again, the action on the liquid that has ceased to circulate is the same as that on the blood still moving through the vessels. I feel persuaded, Gentlemen, that we are in the right way to the discovery of the mechanism of those scourges that desolate certain countries,—of those terrible epidemics which, though known by different names, originate in the same cause. Is any great advance, I would ask, made towards explaining the black vomiting of yellow fever by ascribing it to a *specific gastritis*? If the partisans of inflammation fancy so, what will they say when we demonstrate to them that, by introducing a few drops of putrid water into the circulation, we shall produce an accurate imitation of the symptoms of their pretended specific gastritis; if we show them that decomposed blood, effused through the gastro-intestinal mucous membrane, constitutes the matters vomited? But will they, after this, be convinced that neither irritation nor inflammation have been in action? Perhaps they may, but that they should acknowledge their new-born conviction is too much to expect. I now proceed, in illustration of what I have been saying, to the autopsy of an animal who died in two hours from the injection of a little putrid water into the veins. As life lasted so short a time after the operation we shall probably not find any very extensive lesions. The first incision through the integuments allows us to feel pretty sure as to the fact of the blood being liquefied; observe how it flows from the wound; the muscles present a remarkable punctuated red colouration, such as is often met with in the brain, and is caused by a vast number of small petechiæ formed of extravasated blood; the lung has only retracted very slightly on opening the chest, nevertheless it is scarcely at all diseased; the chief disorders will, no doubt, be found in the abdomen, which I proceed to open. Here I may remind you of a fact to which we have so far met with no exception, namely, that when we inject subcarbonate of soda in solution the thoracic organs are those presenting the disorders productive of the fatal result. You see, in this case, that the mucous coat of the intestines is raised by a deposition of blood in the subjacent cellular tissue; on the surface, too, are large patches of albumen and mucus, the whole forming a very excellent case of gastro-enteritis for the disciples of a certain school; for my part I see nothing in it more than distention of the capillary vessels with blackish fluid blood, the elements of which, having become dissociated, partially transuded through the intestinal tunics. These pathological phenomena are, as we have frequently ascertained, more and more marked the longer the disease has lasted before causing death.

Here is the body of an animal into whose veins twenty-five grammes* of subcarbonate of soda were injected. Death was instantaneous, and must have been caused by profound disorganisation of the lung; and, accordingly, we find that organ distended with liquid blood, which gushes out when an incision is made into its substance; there is, besides, bloody effusion into the pleura. These mechanical disorders constitute what pathologists would call pleuro-pneumonia; in such a case as this they would forthwith set about counting the number of cells unaffected by the inflammation, describing minutely the different shades of colour presented by the lung, and ascertaining the quantity and weight of the pleuritic effusion, &c. But ask them the cause of these disorders, and all you will get from them is the words inflammation and irritation; and after this they pretend to be pathological anatomists! In order to leave no doubt in your minds as to the special influence exercised by the blood when liquefied by the subcarbonate of soda, on the lung, I open the abdomen; you observe that the intestinal convolutions, and the various organs of that cavity, are all of healthy appearance.

ON

ANTEVERSION AND ANTEFLEXION OF THE UTERUS.

By W. Hayle Walshe, *M.D.*, &c.

In consequence of the position the uterus holds in the pelvis, and the laxity of its connections with surrounding parts, that organ is subject to a greater number of displacements than any other. A variety of classifications of these, each based on a different view of the subject, may be easily made; to the following, however, founded on the portion or quantity of the organ which is displaced, I am inclined to give the preference.

A. *Displacements in which the position of the entire organ is changed.*

1. Anterior obliquity.
2. Lateral obliquity.

* The gramme is equivalent to 0.0645 gr.

3. Posterior obliquity.
4. Elevation.
5. Anteversion.
6. Lateriversion (Ruysch).
7. Retroversion.
8. Hernia (inguinal, femoral).
9. Procidentia; prolapsus.

B. *In which part only of the organ is displaced.*

10. Anteflexion.
11. Retroflexion.
12. Lateral partial prolapsus (Ruysch, Obs. Anat. xxiv.).
13. Extroversion of neck (Madame Boivin).
14. Extroversion of internal tunic of the organ (Collomb, Chaussier).

C. *In which the displacement may be either of the entire or of a part only of the organ.*

15. Inversion.

The earliest writers on medicine appear to have had scarcely any definite notions respecting uterine displacements;* Œtius is probably the first author who speaks with a tolerable degree of distinctness of one of the most important amongst them, retroversion. Mercurialis and others followed in the same track, but, as is well known, their hints concerning it were completely lost sight of, and to Dr. W. Hunter is due the honour of originating the series of laborious and minute investigations of which retroversion has been the subject; and it must be confessed, that the state of knowledge on these affections is, even at the present day, far from being satisfactory. If we examine the condition of our literature as respects them, we shall, no doubt, find much ingenious speculation on their causes, their combinations, and their general history, all of which, too, may be frequently available in practice; still, when we proceed to details, we soon learn that a very unequal share of the attention of writers, especially those of our own country, has been bestowed on the different malpositions of the organ. While elaborate articles are devoted to the discussion of the nature and treatment of some of them, others receive a mere passing notice, and the existence of a few, still less fortunate, is not even adverted to. In proof of this assertion I need only mention the fact, that retroversion is treated of at some length in our elementary treatises, whilst the converse state, anteversion, is scarcely described in any of them; indeed, its very existence, as a disease, is questioned (why, it is difficult to conceive) by Dr. Blundell, confessedly one of our highest authorities on such matters.† Anteversion is, it is true, considered with more than usual care in Dr. Fleetwood Churchill's recent and very comprehensive work; but the state called retroflexion is not alluded to, and anteflexion confounded with a perfectly distinct condition, anterior obliquity.* Again, in Dr. Burns' copious treatise the whole subject of anteversion is dispatched in a few lines (8th edit.).

These instances, taken from among many similar ones, suffice to show that certain uterine displacements are not usually considered of much practical importance; but there cannot, I think, be a doubt that this is an error; at least the records of medicine indirectly show that one of those habitually neglected by authors, namely, anteversion, is of very frequent occurrence, while they prove that it is a complaint of by no means trifling severity; and, further, that it is one likely, from the nature of the circumstances under which it often originates, to be mistaken for a simply functional or irritative disorder, though not so serious in its consequences; therefore it is still a complaint of quite as real interest to the practitioner as retroversion.

It is not my intention to attempt anything like a systematic history of uterine deviations, but I shall, for the reasons just stated, I trust, be pardoned by the readers of THE LANCET, for occupying some of its valuable space with a few considerations on anteversion, and an analogous state, anteflexion. They are founded on the analysis of eighteen cases, scattered through different works, the greater number of which had already been collected by M. Ammeline, in his excellent Thesis published in 1827. As scarcely any of these are given with sufficient accuracy of detail (not to speak of the small number of the whole collection), to admit of a correct application of the numerical method, I have preferred general statements to numerical ones, which might mislead the reader, by an appearance of accuracy to which they could lay no real claim. The case reported below I had an opportunity of observing, some years past, in the wards of M. Louis; it is of some value, as there are only three or four, I believe, on record, in which the state of the uterus was established by dissection, and a still less number in which the observation of the symptoms was followed by post-mortem examination.

V. E., ætat. 38, admitted December 16, examined January 9.

Previous History.—Has worked for last three years as charwoman, was before that portress and housemaid. Catamenia appeared at the age of 16, without pain, perfectly regular from the outset in their return; lasted four or five days, without clots, swelling of breasts, or cephalalgia. Has

* The ideas advanced on the subject in the work "De Morbis Mulierum," attributed to Hippocrates, must be admitted to be extremely vague.

† Lectures, p. 261, Lond., 1832.

* On the principal Diseases of Females, p. 265.

had six children, first at the age of 17, last at the age of 23; no change in menses after confinements; no leucorrhœa in their intervals. Has been subject for the last five years to pain near the upper border of the sacrum, after the least fatigue. Her food has been poor in quality, she has not for years been in the habit of eating meat; considers herself ill since Nov. 10, previous to which time she had no sensation of bearing down at the anus, pain in groins, nor white discharge; for a month before, however, the sacral pain had increased, and she had been only very slightly unwell on the two preceding occasions. On that day, while employed in washing, a sudden flow of blood, with large clots, took place per vaginam, without her having made any particular effort. Feeling no pain she continued her work, and has since then had a persistent red discharge, which, for the first month, daily equalled the quantity lost in the same time during catamenia, and has latterly diminished. Inguinal pain at first considerable, now less; for last fifteen days suffers from occasional pricking pain in left thigh; has lost half her former flesh; scarcely ate anything during the first month.

Present State.—Skin dull, but not straw-coloured; decubitus right lateral, as it has been for the last two months; tongue moist, whitish; abdomen indolent and lax, except towards pubis and left groin; the sacral pain still exists, but to a less degree than before; has always been subject to constipation, but defecation unusually difficult for the last two months, and increasingly so; urine in proportion to drink, the patient makes no complaint of difficulty in passing it; no thoracic symptom; pulse 76, small, regular; inodorous vaginal discharge, equalling in quantity about an eighth of what is lost daily during menses. *By speculum.*—Nothing remarkable in state of vagina; impossible to see orifice of uterus. *Examination with finger.*—Natural temperature of vagina; neck of uterus 2½ inches from vulva, broad, unusually hard, and turned backwards; anteriorly towards the pubis a tumor is felt, formed by the body of the organ; on pushing it upwards depression of the neck follows, the patient being in the recumbent position.

Infusion of cocquelicot, for drink.
Gum draught, with rhatany root, in powder, gr. xxiv.
Gummy extract of opium, gr. j. in pill.
Vaginal injection, with decoction of morel and poppy-heads.
Quarter diet.

Jan. 20. Omit the opiate.
Balsam of copaiba, gtt. xx., made into pills.

28. Discharge increased for last six days; the general symptoms are, however, much improved; colour comparatively fresh; no pain; appetite as in health; sleep good; she states that were it not for the discharge she should consider herself perfectly well. Omit copaiba; common diet.

Feb. 12. A new train of symptoms appeared; abdomen greatly swelled, and very tender; violent pain in hypogastrium, first slightly felt on the 9th; bladder not distended; mictions natural; frequent vomiting since yesterday of greenish matter; this is on the increase; no stool for last four days; tongue pale, moist; no cough; pulse 112, regular, very small; discharge almost totally ceased; decubitus dorsal, knees raised; features contracted, and expressive of extreme suffering.

Fifteen leeches to hypogastrium.
Enema of *sulphate of soda*, ℥j.
Solution of tartarous syrup for drink.

13. Vomited almost all her drink yesterday, but nothing since three, A.M.; abdominal tension increased; tympanitis to an extreme degree; the least percussion causes torture; hypogastric pain unrelieved; no typhoid or other maculæ on skin; tongue white, moist, no redness of borders; considerable thirst; enema has not been voided; pulse 126, regular, fuller than yesterday; no anormal sound at precordial region; respiratory murmur distinct there; pulsation of heart feeble; no sound in carotids; respiration 54, deep; auscultation gives normal results; voice a mere whisper; decubitus dorsal, as before.

Repeat enema.
Extract of digitalis, gr. ij. in pills.

14. Tongue as before; continual nausea; vomited twice; two stools, consisting of enemata only; urine natural; pulse 110, regular, pretty full; respiration 48; abdominal parietes almost motionless; tendency to somnolence; emaciation visible; slight discharge per vaginam; skin dry, scarcely hotter than natural.

Bottle of Seltzer water.
Mercurial ointment, ℥j. To be rubbed into the abdomen and upper part of thighs, in half-drachm doses, every hour.
Omit the digitalis.

Expired at nine, P.M.

Autopsy 36 hours after death; weather mild and damp.

Exterior.—Considerable emaciation; muscles well coloured; slight lividity of sides of abdomen; very little cadaveric rigidity.

Abdomen.—Diaphragm raised to level of fifth intercostal space; the tympanitis is found to have been produced by distention of small and large intestine, particularly of the former; convolutions of small intestine glued together by greenish false membrane, but no fluid between them; left colon vertical, brick-coloured, surface mammilated, and covered with a layer of viscid semi-fluid

matter; transverse colon directed obliquely downwards to left iliac spine, whence it ascends to diaphragm, and then redescends, being bound down in this position by false membranes; sigmoid flexure adherent inferiorly; on breaking up the adhesions a quantity of dull red-coloured fluid is discovered, and the posterior surface of the intestine is perfectly black and covered with dark-coloured detritus; in the cul de sac between uterus and rectum is a clot of blood as large as an egg, surface black, not distinctly fibrinous; to account for it there appear to be some vessels open; here, too, are several loculi, with pseudo-membranous walls, of hard white-of-egg consistence, and containing putrid clots; these being removed, a black layer of membrane is found adherent to the peritoneum, which is healthy underneath. *Stomach.*—Peritoneal surface brick-red colour; mucous membrane of natural consistence everywhere, rather thin in great cul de sac, and on posterior surface, non-mammilated, no mucus on its surface; submucous tissue of great tuberosity infiltrated with reddish serum. *Small intestine*—Walls, notwithstanding their distention, thicker than in normal state; mucous membrane pale or greenish throughout, consistence and thickness normal. *Large intestine* contains a quantity of pultaceous greenish matter, especially rectum and sigmoid flexure; very wide all the way to anus; mucous membrane natural. *Kidneys, pancreas,* and *spleen* healthy. *Liver* covered with yellow pseudo-membrane, one line thick and fragile; cohesion of substance somewhat less than natural. *Uterus* flexed on itself at an obtuse angle at the union of its body and neck, in such manner that the fundus, concealed by the bladder, is inclined forwards and downwards, while the neck is inclined backwards to the sacrum, the posterior surface of the body being antero-superior. There is a slight lateral obliquity in its direction, the neck being turned somewhat to the right of the middle line, the fundus towards the left crural arch. The body of the organ, as well as the neck, is hypertrophous; their substance is of a grayish hue and hardened, firm, and resisting throughout, except at the union of those parts, where there is a band of the organ, flattened from before to behind, extremely soft, flabby, and yielding, and corresponding exactly to the angle of flexion. The anterior and posterior walls of the body each measure precisely an inch in thickness; the enlargement of the fundus is in proportion to that of the rest of the organ; the neck is 2½ inches wide, its orifice gaping, borders greenish in colour; cavity in no way remarkable. *Ovaries.*—*Right,* enlarged and divided into cells containing a consistent puriform fluid. *Left,* also divided into loculi, filled with a citron-coloured serous fluid; a small reddish clot in one of them. *Vagina* 5½ inches wide; walls three lines thick, softened at its upper part. *Bladder* much distended; internal surface irregularly red; submucous tissue of part corresponding to uterus infiltrated with reddish serum.

THORAX. — *Pericardium* contains ℥ij of straw-coloured serosity. *Heart* of ordinary size; black firm coagula in right ventricle; walls one line thick; pulmonary valves livid-coloured; left ventricle contains two small blackish clots; aortic valves pale. *Aorta* pale, yet it contains some fluid blood. *Pulmonary artery* livid. *Lungs.* — *Left*: Upper lobe perfectly healthy; slight engorgement and diminished consistence of inferior lobe; a few old adhesions posteriorly. *Right*: inferior and middle lobes more engorged than on left side; pleuritic adhesions; bronchi of lower lobe rather livid; mucous membrane gives strips eight lines long; no fluid in either pleura.

ENCEPHALON.—Dura mater moderately covered with blood; posterior meningeal veins gorged; membranes easily separable from brain; very slight sub-arachnoid infiltration; rare punctuated injection about centrum ovale; pons Varolii perhaps somewhat less firm than natural; in all other respects brain and cerebellum perfectly healthy.

Whatever observations this case, defective as it unfortunately is, in many points, may suggest, will be introduced into the general description of the complaint, to which I now proceed.

ANATOMICAL CHARACTERS.—As M. Ammeline justly observes, anteversion may exist in several different degrees; but all of them may be referred to two classes. In the first, or commencing stage, the uterus is placed horizontally in the pelvis, with its fundus turned directly forwards to the bladder, and occasionally sunk into its coats; while its orifice looks towards the cavity of the sacrum. In the second, or more advanced stage, the posterior surface of the fundus looks somewhat forwards, the anterior downwards and backwards; that portion of the organ is placed in the cul de sac of the peritoneum between the uterus and bladder, while the neck is tilted proportionally upwards towards the sacro-vertebral angle.

In anteflexion (a very remarkable example of which displacement has been described and figured by Madame Boivin) the body of the organ is bent forwards on the neck in such manner that its posterior wall becomes antero-superior, assuming very closely the position observed in the second stage of anteversion; but the neck retains its natural place in the centre of the vagina. The deviation of the uterus in the case I have reported above, was consequently, as is seen by the annexed engraving, a sort of combination of the two conditions, a variety which appears to have been more frequently met with than pure anteflexion.

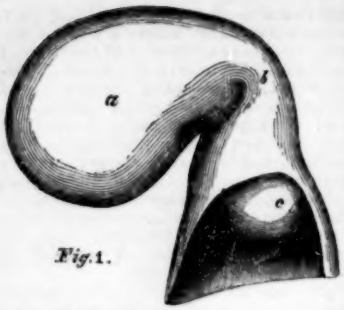


Fig. 1.

a. Body of the organ.

b. Angle of flexion, with flattening of the tissue from before to behind.

c. Inferior portion of neck.

The dotted lines show the position of the neck in anteflexion.

The distance of the displaced uterus from the vulva varies considerably in different cases, more especially as regards its neck. Though that part of the organ is generally said to have been "very close to," or within "half an inch or an inch" of the genital fissure, it is described in others as being placed out of the reach of the finger; in one instance, we are informed, it was impossible to reach it with Madame Boivin's instrument invented for the purpose; the fundus is very frequently much lower in the pelvis than the neck. This shows that anteversion may be combined either with prolapsus or elevation; as, also, that Désormeaux was in error in looking on it as a mere variety of procidentia. There is occasionally a certain degree of lateral obliquity; for instance in M. Louis' patient, and in Morgagni's case, the earliest on record.*

The dimensions of the uterus varied considerably in the cases under consideration. It was theoretically laid down by Leuret, and the opinion has been shared by others, that hypertrophy of the anterior wall was the immediate cause of the falling forwards; but this hypothesis has been disproved by every anatomical examination that has been made: in the case observed by myself both walls were equally thickened. Nor is it true that enlargement of the organ is at all necessary for the production of the displacement; far from this, it is quite as frequently described, in the eighteen cases, to have been unusually small, as of overgrown size. In the majority of them, it is true, no note is made of the point; but this is presumptive evidence that the dimensions were natural or nearly so.

In Madame Boivin's case of anteflexion the tissue of the uterus was very dense and of blackish colour. It is probable this condition was quite unconnected with the malposition, and ascribable to amenorrhœa and the debauched habits of the patient. The excellent observer who has described the displacement looks on it as congenital in this instance; she is in all likelihood correct, as the symptoms it usually produces were altogether absent. In this case, as well as in Morgagni's, the vessels of the broad ligaments were varicose and engorged with black blood. The Italian pathologist states that the vagina of his patient was "too broad and too long for her stature;" the width of its cavity, and thickness of its walls were considerably greater in the female observed by myself than is usually the case. Among the causes of anteversion some writers have ranged unnatural shortness of the round ligaments; and in Levret's celebrated case, in which the anteverted fundus was mistaken for an encysted calculus in the bladder, such a condition of those ligaments is stated to have existed. This is the only foundation that I have met with for the theory; it is a plausible one enough, but its correctness may be looked on as still *sub judice*.

There were very clear evidences, in the case I have given, of chronic metritis, in the grayish hue, induration, and increased size of the organ; the softened state of the flexed part must also be looked on primitively (whether we be further disposed to regard it as the cause or an effect of the flexion) as a result, though it is certainly a rare one, of the chronic form of uterine inflammation. In Madame Boivin's case this latter peculiarity is not stated to have existed, but when the uterus was straightened and then left to itself, it instantly resumed its abnormal bend. A somewhat analogous appearance is occasionally observed in the liver, in those cases of commencing longitudinal flexion, in which the organ seems divided into two parts by long-continued constriction. Here the normal tissue seems absorbed and replaced by a kind of fibrous substance.

M. Louis' is the only case in which any unusual appearance has been found in the ovaries; it was no doubt a mere coincidence therein.

(To be concluded next week.)

* De Sed. et Caus. Morborum, Ep. xlvi.

GLANDERS IN THE HUMAN SUBJECT.—The identity of glanders in the human subject and in the horse has been recently demonstrated by M. Rayer. A patient in M. Roux's ward, at the Hôtel Dieu, had contracted glanders while examining a diseased horse. M. Rayer took some pus from the patient, and with it inoculated a healthy ass. The animal died with all the signs of acute glanders, nine days afterwards.

ON THE

PHYSIOLOGY

OF THE

MECHANICAL ACTION OF THE HEART.

To the Editor of THE LANCET.

SIR:—The following paper, read on the 7th inst. before the Manchester Medical Society, may, perhaps, be deemed worthy of a place in your valuable periodical.

Your obedient servant,

JOHN GOODMAN, M.R.C.S.L.

112, Greengate, Salford,
Nov. 20th, 1838.

After stating the necessity of attending to the figure, relative situation, size, texture, &c. of the heart itself, as well as the structure and integrity of its surrounding walls, in attempting an explanation of its *modus operandi*, I proceeded, cursorily, to describe its figure and relative situation.

Size.—I next stated that the size of the heart was subject to considerable variation, but that to agree with the views stated in the following pages it is necessary that the heart should be of such a magnitude, in every animal, that the ventricles, during a perfect dilatation, should very nearly occupy the whole of the antero-posterior diameter of the pericardium, the empty and compressed auricles occupying the remainder; or, in other words, that the fully dilated ventricles and compressed auricles should completely fill the whole of the pericardium.

Texture.—The ventricles of the heart are admitted, at all hands, to be decidedly muscular. The auricles are not, however, so unequivocally muscular, except at their appendices, as to remove all doubt on the subject; and it is my intention to show that, in the action of the heart, there is no necessity for muscular fibres in the parietes of the auricles. The natural muscular arrangement of the ventricles is such, that the state of rest is identical with the state of dilatation of the ventricles. If the auricles were also admitted to have their states of muscular contractions and repose, the state of rest would be during the contraction of the ventricles, and *vice versâ*.

The surrounding walls of the heart may be divided into two,—the external, or bony arch, and diaphragm; the internal, or pericardium.

The outer walls, or bony arch, possess such strength of material, and peculiar convex form, that the pressure of the atmosphere, generally understood to be 15lbs. on every square inch of surface, is firmly and perfectly sustained by this mechanism, and the contained viscera are protected from its influence. Not only does this protecting arch resist the force of external pressure; but it is also capable, to a certain extent, of expanding itself, and producing a partial vacuum within its parietes. The diaphragm, or floor of this cavity, is also able to resist the power of atmospheric pressure, although of muscular construction. The bony arch, by its unyielding structure, presents itself to the oppressing forces with the firmness of the oak, in an arched and resisting form, while the more yielding diaphragm, like the willow before the wind, bending beneath the atmospheric pressure, presents a concave, but still resisting and equally protecting surface.

Internal Walls.—The internal wall surrounding the heart, or pericardium, is a strong fibro-serous membrane, &c., and will also resist to some extent the pressure of external viscera. To illustrate the extent to which the lungs might be capable of encroaching within the pericardium, I removed the heart, during a post-mortem examination, through an opening in the diaphragm, without injuring the bronchi, sternum, ribs, spine, or pleuræ. I inserted a tube in the trachea, rendered it air-tight, and inflated the lungs to the utmost extent, but the pericardium remained, apparently, of an equal capacity to that observed before the commencement of the inflation.

The next point is, that the distance between the spine and sternum, or the antero-posterior diameter of the pericardium, in ordinary respiration, does not vary considerably. In one individual, measured with callipers from the spine to the cartilages of the sixth rib, or the point where the apex of the heart was then beating, the chest only expanded one-eighth of an inch in an ordinary inspiration. In another individual it varied from one-eighth to one-quarter of an inch in ordinary inspiration, and at the sternum three-eighths of an inch during a full inspiration. In no instance, however, does the chest ever fall in during the contraction of the heart, the increase and decrease in the antero-posterior diameter of the pericardium being alone altered by respiration, and not by ventricular contraction. The first movement which takes place in the adult heart is the contraction of the conjoined ventricles; but as there are some evident facts connected with muscular and material contractions it may be well first to take these into consideration.

1st. That muscular fibres are invariably found shortened during their contraction, and, at the same time, not at all, or inconsiderably increased in volume.

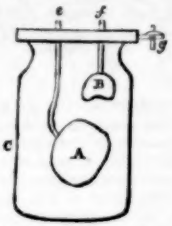
2nd. That in whatever situation a number of muscular organs are put in motion, an increased supply of blood to and from the neighbourhood of the muscles so exercised, is a commonly acknowledged phenomenon.

3rd. If we look around to Nature for examples of the shortening or lengthening of

any material substance, we find, in some instances, that bodies, as the metals, &c., lengthen by the application of heat, and shorten by its removal; that others, as wood, &c., become distended by the absorption of fluids, and contract again when dry; others again, as the sensitive plants, appear to be capable of contracting and expanding by their peculiar structural arrangement. It is admitted, at all hands, however, and is one of the primitive, and most established laws of the material universe, "That in no one instance do the ultimate particles, or elements of matter, ever penetrate one another;" and, in the generality of instances, the encroachment of a subtle fluid between the particles or fibres of bodies produces the distention, and by the removal of which the contractions of bodies is the immediate consequence.

Finding, therefore, that the supply of blood to a muscle, in a state of action, is always in increased quantity, and the return in the same ratio; that the length of the muscle so acting is shortened according to the extent of that action, there being at the same time, apparently, no increase in the volume of the same to account for the diminution in length; and for several other reasons, I have arrived at the purely speculative conclusion, that the primitive muscular fibre is composed of an extremely minute cellular structure of, probably, globular cells, arranged longitudinally in its substance, and for the purpose of receiving and being distended by arterial blood. That the fibrous wall surrounding each sanguineous cell is possessed of contractile power, under the immediate influence of nervous energy; the contractile appearance observable in the corrugations of the skin by the application of lime-water, acids, astringents, &c., being sufficiently extensive to assume an analogous condition. That, upon the contraction of the wall of each cell, the contained blood is wholly or partially ejected into the veins, producing in them an increased flow (as is particularly recognised in venesection), and as the cells are disposed longitudinally, so the lessening in volume of each cell will be followed by the shortening of each fibre in which they are arranged, and this effect, in the aggregate, is the sole cause of the visible and extensive contractions of the muscular system. Suppose, then, the contraction of the ventricles to take place, upon the volume of blood contained within them, thus discharging it at once through the aorta and pulmonary vessels, the consequence must be a decrease in the size or volume of the conjoined ventricles. This diminution in volume of the ventricles is the main point in the physiology of the heart's action, and one of two circumstances must be the instantaneous result: either the anterior walls of the pericardium must give way, and follow the contraction of the ventricles, or a vacuum must take place betwixt the apex of the heart and the sternum it has now relinquished. That the walls of the chest do not give way in the adult and close upon the contracted ventricles is a well known fact, therefore a vacuum ensuing betwixt the apex of the heart and the anterior wall of the pericardium, we shall have two forces, and those of no ordinary magnitude, operating to fill the vacuity.

Fig. 1.

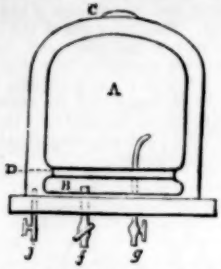


In this closed and air-tight glass vessel C, which is intended to represent the pericardium, and in which are inserted the two Indian-rubber bags A, B, totally unconnected with each other, no decrease in size can take place (by exhaustion through the tube *e*) in the bag A, without an immediate dilatation to a similar extent of the bag B, by the pressure of the atmosphere to which B is exposed by the open tube *f*. By removing the contents of the vessel A, we produce a partial vacuum in the vessel C; the loss can alone be supplied by the tube *f*, during the exhaustion of A, and consequently we have the pressure of the atmosphere exciting its influence to fill B, to an extent equal to that to which A has diminished. A is intended to represent the conjoined ventricles, and B the conjoined auricles of the heart. During the contraction of the ventricles, and existence of a vacuum in front of the apex of the heart, then, we have the atmospheric pressure exerting its influence upon every vein throughout the system, to force their contents into and to produce the dilatation of the auricles. We have also a second power aiding and assisting the atmospheric pressure, viz., the *vis a tergo*, or force which the venous blood derives from the arterial system.

This apparatus, which consists of an air-tight glass vessel, or pericardium, C, contains two oiled bladder vessels; the one, A, is intended to represent the conjoined ventricles, and communicates by the curved tube, and the cock *g*, with the exhausting syringe of an air-pump. The bladder B is intended to represent the auricles, and communicates with the atmosphere when the cock *f* is open; the cock *j* opens communication between the atmosphere and internal of the pericardium. If we now shut *j* and *f*, and exhaust the bladder A, so

as to completely empty it, there will be a vacuum in C to the extent occupied at pre-

Fig. 2.



sent by the bladder A. If we then open *f*, and allow the pressure of the atmosphere to communicate with the bladder B, it will become immediately distended, and will, in so doing, propel forward the septum D and exhausted bladder A to the upper part of the vessel. In this manner the conjoined powers produce, by the rushing in of blood and distention of the auricles, the projection forwards of the auriculo-ventricular septum, together with the contracted ventricles through the vacuitous portion of the pericardium; thus filling the vacuum and producing the impulse of the heart, by causing its apex to strike the posterior surface of the cartilage of the sixth rib, in a similar manner that the piston in the condensing engine is, by the power of the steam, carried with terrific violence through the vacuitous portions of the cylinders. Having, then, this force of blood rushing into and distending the auricles, their parietes must, of necessity, become distended to the very limits of the vacuum; and as a bottle blown at the glass-house assumes the identical form of the mould in which it is enclosed, so the dilated auricles will at once assume the exact form of the posterior portion of pericardium hitherto occupied by the base of the ventricles with collapsed auricles, at once accounting for that peculiar form of the appendices which has procured for them the epithet "aures caninæ."

In examining the base of the heart, we find the origin of the aorta in the exact centre; immediately above it that of the pulmonary artery, and on each side, surrounding the bases of these vessels, we have the auricles and their appendices beautifully filling up and covering the whole base of the heart, for the purpose of producing an uniform distention posteriorly, and filling up the space just alluded to. The use of the auricular appendices appears to be intended chiefly for the purpose of giving free motion to the arch of the aorta. If the entire form of the auricle were that of a bag without appendix, united along its whole extent to the base of the ventricles, to its fellow, and to the origin of the aorta, which must of necessity pass through the same, we should have a decided extension or retraction of this firm and important vessel, to the extent of at least one inch, during the dilated and exhausted condition of the auricles, seeing that it would have to extend, at all periods, from the back of the auricles to the auriculo-ventricular septum. But by this beautiful arrangement of the appendices unconnected to the aorta, pulmonary artery, or ventricles, we have a facility afforded for the use of the whole arch of the aorta during the movements of the auriculo-ventricular septum.

Dr. Hope (I believe) declined giving an opinion upon the cause of the impulse of the heart, but states, page 42,—"When the heart of an animal, as a frog, rabbit, dog, turtle, &c., is detached from the body before organic life is extinct, and placed upon a table, it continues to act, and each contraction elevates the apex;" and, page 43,—"During the state of relaxation the heart lies collapsed and flattened, with large extent of surface applied to the table; on contracting it starts up, and, assuming a more rounded form, is sustained by a comparatively small point of contact, and the apex is consequently elevated." In another page,—"In proportion as the ventricles contract to their extreme, the apex is thrown more and more forward by the auricular distention, advancing in the same proportion to its extreme."

According to Bertin,—"The dilatation or distention of the auricles is the cause of the heart being carried forwards, so as to beat between the fifth and sixth ribs."—p. 30.

Magendie says, vol. ii., p. 70,—"The displacement forwards of the heart, during the ventricular systole, has given rise to a long and obstinate controversy. Some pretend that the heart shortens during contraction, others maintain that it lengthens, and that it ought necessarily so to do. Bussnell, however, showed, that if the heart lengthened in the systole, the mitral and tricuspid valves would not be able to shut the auriculo-ventricular openings. Senac showed (says he) that this phenomenon depended on three things,—

"1. The dilatation of the auricles, which is made during the ventricular contraction.

"2. The dilatation of the aorta and pulmonary artery, in consequence of the introduction of the blood propelled thither by the ventricles.

"3. The erection of the arch of the aorta produced by the contraction of the left ventricle."

That the auricles of the natural heart are sufficiently dilateable to produce a projection forwards of the apex, is exhibited with facility by tying up all the vessels proceeding from the same, except one of the venæ cavæ, and inserting a tube into the latter, the two auricles being made to communicate

with each other. This heart (one exhibited) has been thus dealt with, and by blowing into its conjoined auricles you observe an immediate starting forwards of the apex. The contraction of the animal ventricle is of necessity much more rapid than the dilatation of the auricle, and, in consequence, the vacuum is formed before the auricle can possibly be distended, which is evinced by the blow on the chest, for it is impossible a blow can be struck unless the hammer be first raised from the anvil. The power, also, by which the ventricles contract is infinitely superior to that by which the auricles are filled. For instance, the ventricles propel the sanguineous fluid against the whole pressure of the atmosphere throughout the body, overcome the whole elasticity of the arterial system and animal structure, and after all produce the vis a tergo of the veins.

The next subject that demands investigation is the question—Do the auricles contract by their own muscular power or not? It has already been sufficiently shown (fig. 1) that the auricle is dependent upon the ventricle for its increase or decrease in volume. We find, if we open the valve of fig. 2, thereby opening the bladder B to the action of the atmosphere, and exhaust or dilate the bladder A, that no change can take place in the volume of the bladder A without a corresponding one of the opposite character immediately ensuing in the bladder B; if A empties, B fills; A filling, B rapidly decreases, &c. It is apparent, therefore, that there is no necessity at all for muscular fibre in the parietes of the auricles, for facilitating or producing the action of the heart. It is, however, evident that the *appendices* of the auricles do possess true and well developed muscular fibres, and it appears that they are situated here for the purpose of opposing obstruction to the emptying their contents. Being loose and floating in the pericardium they might probably become entangled, and if we also pay attention to the manner in which they are emptied we shall see here that their distention would be produced at each diastole of the ventricles by pressure on their outlet.

The next object for consideration is the third and last action performed by this organ, viz., the apparent relaxation of the muscular fibres, and distention of the ventricles, at once receiving the contents of the auricles, and repelling the auriculo-ventricular septum to its normal situation. Some authors attribute this phenomenon to the elasticity of the muscular structure of the ventricles; others, again, affirm that the distention of the ventricles is produced by the muscular contraction of the auricles. When we consider, however, that the dilatation of the ventricles occupies but one-fourth the period required by the whole motion and rest of the heart; that it is reported, indeed, to be performed in half the time required for the contraction of the ventricles, and is considered instantaneous, I think the former of these conclusions, although by far the most probable, yet greatly inadequate to account for the dilatation of the ventricles. I have, therefore, again adopted a speculative conclusion, which, if my view of muscular contraction, as before stated, prove correct, will at once establish the cause of this phenomenon; it is, that the aorta and great vessels being distended by the contents of the ventricles during their contraction, immediately, or on the cessation of the ventricular systole, either by elasticity or muscular contraction (hitherto undecided), project their contents forcibly into every communicating artery throughout the system, and in so doing inject, as the first branches given off, the nutritious arteries of the heart itself, and, with a force, the immediate echo of the ventricular contraction, distend the muscular fibres, and produce the dilatation of the ventricles. We will now take into consideration what would be the result of removing a portion of the walls of the pericardium for the purpose of watching the action of the heart, a method always resorted to by experimentalists who have written upon this subject. If we open the orifices *g* and *j* of fig. 1, or fig. 2, so as to let the atmosphere into the pericardium, and then attempt to fill the auricle by exhausting the ventricle, or to empty the auricle by filling the ventricle, we find that there is now no change in the auricle corresponding to that of the ventricular movement, the pressure of the atmosphere being equally active within and without the pericardium, and no longer assisting in the propulsion forwards of the apex. If we remove the glass pericardium entirely, and apply a dilating power to imitate the vis a tergo, and by this means produce the distention of the auricle, we find that as long as this power is now applied the ventricle may be exhausted, or contracted, to any extent, without affecting in the least the fulness of the auricle. It is, therefore, very evident that the heart *cannot* be *seen* in its natural state of action after the pericardium has been punctured; and, in corroboration of this statement, we will refer to the words of Dr. Hope and others, when describing the motions of the heart as seen during experiment.

Dr. Hope says, "The auricle never emptied itself, and its contraction was always very inconsiderable; the anterior edge and surface were seen to retract with rather a sudden motion, but as the extent of the motion was very inconsiderable it had the appearance of being feeble."

Again, page 18, he remarks, "I had observed in the frog, when the heart was acting slowly, the ventricular systole was followed by a diastole and partial distention with blood, independent of the auricular contraction, and remained quiescent till

again stimulated by auricular contraction." In another page, 31, he says, "The movements of the ventricles, with their corresponding sounds, continued perfect while the auricles were motionless."

Between page 31 and 36—"The auricles evidently contract before the ventricles, and are instantly followed by the systole of the latter; there is an interval of repose distinctly between the ventricular diastole and auricular systole. Such (says he) were the phenomena observed during the short periods when the motions of the auricles were regular, but for the most part there was either no perceptible motion in them, or the motions were irregular, and bore no relation whatever to the ventricular movements."

In further experiments he remarks, "The auricles contract very slightly, and principally at the appendix, the motion running vermicularly into the ventricular systole." Again, "When several irregular ventricular contractions followed each other rapidly, the corresponding diastoles were attended with a slight retraction of the auricles most conspicuous at their sinuses. This phenomenon proceeded from the increased suction of the blood by the ventricles."

At page 36—"The auricles are constantly full, their motions ranging between fulness and distention. Mr. Brodie (now Sir Benjamin Brodie) also testifies," says Dr. Hope, "that he never found the auricles completely empty themselves, nor did he in dogs, rabbits, &c., *ever* observe in them any regular systole, corresponding to and alternating with that of the ventricles. He observed slight contractions of the auricle, especially of the appendix, for one of the ventricle."

Laennec remarks, "The draught of blood from the auricles during the diastole causes the slight retraction observable at this moment."

Magendie speaks of "A decided contraction of the auricles, alternating with that of the ventricles, and expelling, by contraction, the blood they contained."

The use of the auricles appears to be to give facility to the ventricular contraction by becoming distended partly during, and immediately after, the systole of the ventricle, and thus preventing the formation of so great a vacuum as would necessarily result from the lessening of the ventricle. If the piston of a common pump, for example, whose column of fluid was only two yards in depth, were raised, water not being allowed to follow the stroke, the force required to work the piston would be equal to the height of a column of fluid thirty feet, or five times as great as if the water followed freely the rising of the piston. The auricles are also of considerable use in affording a facility for the immediate filling of the ventricles, receiving, at each contraction of the latter, a supply of blood exactly equal to that parted with by the ventricles, and acting the part of a true reservoir. The advantages of a knowledge of the true modus operandi of the heart must certainly throw considerable light upon the diseases of this organ. According to the views herein adopted we have, in hypertrophy, the more rapid and energetic contractions of the too powerful ventricles, producing a greater remove of the apex from the parietes of the chest, *i.e.* much more rapidly than the auricles can be filled; a greater vacuum is the result, and, consequently, a greater intensity of impulse, in ratio according to the extent of the disease. In dilatation, or debility of the ventricular walls, from any circumstance, the feeble contraction of the ventricles will produce a withdrawal of the apex so deliberately that the combined powers fill the auricles as rapidly as the former contract; the apex will, therefore, remain at its destination during the whole action, and no impulse will be produced. In cases of apparent death and fainting, also, where the heart cannot be felt, and yet life remains, a similar mode of reasoning will account for the continued and imperceptible action of the heart upon the same principle.

I will now briefly, in conclusion, sum up, in a condensed form, the principle propositions which I have been attempting to lay before you, the first of which is,—That the external walls of the pericardium are capable, to a certain extent, of sustaining the pressure of the atmosphere, and protecting their viscera from the influence of its action;—That the full ventricle and empty auricle, or the heart, as it is generally met with, occupies naturally the whole of the pericardial space;—That the ventricular contraction produces a decrease in the volume of the heart;—That this decreasing in volume produces a momentary receding of the apex from the sternum, &c., and, as the latter does not follow the contractions of the ventricles, causes a momentary vacuum in front, or the fore part of the pericardium;—That the pressure of the atmosphere, and the power termed vis a tergo, jointly produce a rushing of blood into the auricles at the base of the heart, and thus propel it forwards to fill the vacuity, and, in so doing, the apex strikes the pericardium behind the cartilage of the sixth rib, producing what is termed the impulse of the heart;—That there is no necessity for muscular structure at all in the auricular walls for the accomplishment of either their contraction or dilatation, except that of the floating appendices;—That contraction and dilatation of the auricles take place in the living animal to the same extent as in the systole and diastole of the ventricles, but that these changes cannot occur in a natural manner after the opening of the pericardium, and, therefore, that any deductions, depending upon the *seen* action of the heart, must possess an erroneous tendency.

TUMOURS OF THE TENDO ACHILLIS.

To the Editor of THE LANCET.

SIR:—Permit me, through the medium of THE LANCET, to direct the attention of the medical public to a peculiar affection of the tendo Achillis.

About seven years since a coachman, of irregular dissipated habits, applied to me to relieve him from a tumour, about the size of a pigeon's egg, which occupied each tendo Achillis, about three inches above the os calcis, painful on pressure; the tumours materially impeded his march. He could not recollect when they first appeared, but represented them as very slowly increasing. I advised rest and the application of leeches. The punctures were followed by slight erythema. The patient insisted upon taking his usual journey of about forty miles, during the course of which he made no fewer than thirteen applications to his favourite brandy-bottle, by way of "keeping out the inflammation;" the result will be readily foreseen: a formidable erysipelas set in, and the man's life was saved with much difficulty. Amongst the means employed to arrest the inflammation was the nitrate of silver, which I purposely carried to vesication over the seat of the tumours. I soon after lost sight of the man, but, upon inquiry, about three months ago, I found that they had resisted all means used for their removal.

I recognised the same affection in an amiable and accomplished physician, about three years since. Here it commenced on the right side, and, after a few months, appeared in the left tendon, rather below that on the right. He employed anodyne and discutient liniments, and confined himself to the horizontal posture; but the tumours are nothing abated in point of size, though only occasionally troublesome from tenderness.

More recently I have been consulted by two persons of this neighbourhood for the same affection, and am disposed to think that it is not an uncommon one, possibly not confined to the tendo Achillis. The only authority whom I can recollect as noting the disease in question, is Mr. Mayo. In his late work on Pathology he gives an illustrative case, without comment:—

"A gentleman, æt. 32, after taking more exercise than usual, observed a swelling of the size of half a bean, on the inner and anterior surface of the tendo Achillis, which occasionally caused pain in walking. Half a year afterwards a similar swelling formed in the other tendo Achillis, at the same place: the tumours were situated about two inches above the os calcis. He applied a solution of the oxymuriate of mercury in alcohol, which produced soreness of the skin without making, at the same time, any sensible impression upon the swellings, and then he thought no more about them; they still continue, but are something less in size; and although sensible, so as to be rather painful if pressed, they are otherwise without sensation."

Mr. Mayo's case differs somewhat from *mine*; the tumours would seem to be *on*, rather than *in*, or surrounding the substance of the tendo Achillis, and also to have succeeded violent exertion, neither of which was the case in my patients.

I cannot entirely divest my mind of an impression that these tumours are, in someway, connected with gouty action, yet I shall abstain from offering any hypothetical opinion about them. If any of your numerous readers have satisfied themselves upon the point by actual inspection, as men with superior advantages for studying morbid anatomy may well be supposed to have done, I should feel personally and particularly grateful for some enlightenment in regard to their nature and constitution, as also under what empirical treatment they may have been found to subside. There would almost seem to be something foreign and impertinent in crude speculation upon a point which may be so readily, and probably has been, determined in the dead-house or dissecting-room.

Trusting to your well-known liberality to publish this communication in the most suitable form, I have the honour, to be Sir, your obedient, humble servant,

REGINALD BURRIDGE, M.D.
Physician to the Taunton and Somerset Hospital.

Taunton, Dec. 17th, 1838.

HYDROCYANIC ACID IN GASTRO-ENTERITIS.

To the Editor of THE LANCET.

SIR:—I do not remember ever having observed, in any work, hydrocyanic acid recommended in peritoneal and gastro-enteric inflammation. Within the last few months I have been called upon to treat several cases of gastro-enteritis, and, after bleeding the patients as largely as I considered advisable, I began with, and repeated, doses of calomel and opium, at the same time administering two tablespoonfuls of the mist. salin. This treatment not mitigating that invariable symptom, the vomiting, I was induced to try the hydrocyanic acid, in small and repeated doses, and in every case, I am happy to say, with decided advantage.

The first two or three doses were generally vomited, but a few subsequent doses commonly succeeded in lessening all the urgent symptoms, reducing the pulse, and so far allaying the irritability of the stomach as to enable it to retain a mixture containing liq. ammon. acet., digitalis, and antimony; and

this even in cases where a small quantity of cold water had produced a severe paroxysm of vomiting. After the bleeding, whether local or general, I generally administer an emollient enema, and give three drops of the acid, in about half an ounce of camphorated mixture, every hour and a half, until the vomiting is entirely subdued. Should the pain in the region of the abdomen continue, I foment with warm turpentine.

My object is merely to show the advantage to be derived from an early use of this medicine in inflammatory diseases of the stomach and bowels. Your obedient servant,

W. J. M.

Northampton, Dec. 8th, 1838.

APHTHA IN WOMEN

DURING LACTATION, AND ITS CURE BY NITRATE OF SILVER GARGLE.

To the Editor of THE LANCET.

SIR:—In requesting the insertion of the following observations I am actuated by a desire to make public my experience on the utility of the nitrate of silver in a new form, and a different disease from any in which it has hitherto been used, in order that other practitioners may give the remedy a trial, and test its efficacy in the like form and complaint. I am, Sir, your obedient servant,

JOHN KILBURN.

Auckland, Dec. 21, 1838.

Aphtha, whether it make its appearance in the adult or the infant, is, for the most part, dependent on disorder of the digestive organs, or a cachectic state of the whole system. In the infant it is very usually met with during the first month or two, when the mother may have partaken of a more stimulating diet than is consistent with her lying-in state. In the adult, also, it is the offspring of intemperance, and most frequently makes its appearance after the excessive use of intoxicating liquors. But I have occasionally observed a species to occur in the female, while suckling, where it has proved remarkably obstinate. This kind appears to be attended with debility of the whole system, and is more dependent on that than on functional or other disorder of the digestive organs, which the first view of the complaint might lead one to suppose. It is on this account that mercurials, or any other things which have a weakening tendency, are injurious, excepting when used with the greatest circumspection. It usually commences with anxiety, soreness and uneasiness about the precordial region; the appetite fails, the muscles waste, and the complexion becomes sallow, but, notwithstanding this, the lips are blanched, and the tunica adnata of the eye is pearly white. In some cases the inside of the mouth and the tongue present a red appearance, with here and there a small ulcer. In other cases the tongue has the appearance of a smooth piece of veal. The smarting is very excessive, and when anything is taken into the mouth it is described to resemble scalding with a hot liquid. The patient, therefore, is unable to take solid food, and liquids only with the greatest suffering. In this way the complaint persists for weeks, perhaps for months, the symptoms sometimes being mitigated and sometimes aggravated, the intense soreness of the mouth being the predominant symptom. I have seen no exact description of this complaint, as being peculiar to women who are giving suck, but it most resembles the cachexia chronica of Dr. Thomas, cases of which he had witnessed in our West Indian colonies, in males as well as females.

As the majority of the cases which have come under my observation occurred in females who were nursing their children, I am inclined to think that the disorder is brought on by the debility which is induced in the system of those mothers who, from any cause, do not take sufficient nourishment at such times. My opinion in this respect is further strengthened by the fact that many women have recovered after weaning their children, without the use of any remedial measures, they having before used every known means without avail.

A case of this disorder came under my care in the person of a Mrs. Hodgson, in whom the precordial symptoms took place a month or two after confinement, but who did not apply for any medical aid until about five months after that event. The mouth had become so sore that she was unable to take anything into it without suffering the greatest agony. Without any febrile affection she was in a state of great emaciation, and no doubt if she had not obtained relief she would have sunk under the weakness induced, as it were, by hunger; for even after the appetite was restored by the use of tonics, she still could not masticate anything, in consequence of the pungent smarting of the mouth. In the meantime I made use of all the cooling and astringent gargles recommended by authors in such cases, beginning with a solution of the borate of soda, and going through the whole catalogue of medicines, until I arrived at the sulphate of copper, without any amendment.

At last the known sedative qualities of the nitrate of silver, which had been developed in its sanative effects in some cases of epilepsy, as well as in erysipelas, and in ulcers, suggested to my mind its probable good effects in this species of aphtha. I thought that half a grain, dissolved in one ounce of water, would be the most likely proportion in which to use it, and I found

that quantity exactly to answer the purpose. The first application soothed the pain, and she continued to gargle the mouth, at the same time that she was still giving suck, until she was completely relieved; and being then able, with the assistance of tonics, to take sufficient nourishment, I saw her regain her wonted health and strength.

Since the above case occurred I have used the same kind of gargle in some other cases, of a similar nature, with the best effects; I am, therefore, led to hope that it may be found useful in other hands as well as mine, in like cases. I have also used the same gargle in a case of deep ulceration of the throat with decided benefit.

PROCEEDINGS OF PUBLIC BODIES IN NOTTINGHAMSHIRE,

RESPECTING

GRATUITOUS MEDICAL ATTENDANCE.

To the Editor of THE LANCET.

SIR:—I herewith forward to you an account of the proceedings of a special meeting of the Nottingham Branch of the BRITISH MEDICAL ASSOCIATION, convened for the purpose of taking into consideration a letter from the Salford and Pendleton Dispensary, a copy of which is also annexed. I remain, Sir, your obedient servant,

HENRY C. ATTENBURROW, Hon. Sec.
Nottingham, Dec. 18, 1838.

To the Senior Physician of the Nottingham General Hospital.

Sir:—I beg leave to call your attention to the annexed resolutions of the Medical Board of the Salford and Pendleton Dispensary, in doing which it is necessary to explain to you that an application has lately been made by the Board of Guardians of the Salford Union, to the Weekly Committee of this institution, offering to subscribe 50*l.* per annum, on the consideration that the out-door medical aid and medicines required for all the paupers be given by the dispensary, excepting to such as may come under the casual notice of the two surgeons—the one of the Salford, the other of the Pendleton workhouse, the in-door paupers of which they (the two surgeons) are to attend and provide medicines for.

The decision of the Weekly Committee has been postponed until Friday, the 21st instant, to give time to make inquiry on the subject, and the Medical Board have thought themselves called upon to consult the feelings of the members of the profession who may be connected with charitable institutions, in order that, in giving their advice to the Weekly Board, they may be able to represent the opinions of those whose counsel and assistance they are now asking; and, as the object aims at benefitting the profession generally, and not the medical officers of the institution in any ample measure, they will be glad to receive the result of your candid and unbiassed judgment.

You will perceive that it is important that the Medical Board may be favoured with your answer by not later than the 19th instant, in order that they may have time to confer on the collected letters by the 20th, which will be the day immediately preceding the meeting of the Weekly Committee, at which the Board will have to report on the subject. With every feeling of respect, I beg to remain your obedient, humble servant,

GEORGE GARDOM, Chairman.
Salford and Pendleton Royal Dispensary,
Manchester, Dec. 12, 1838.

"*Dec.* 12, 1838. *Salford and Pendleton Royal Dispensary.*

"At a special meeting of the Medical Board, held this day, the following resolutions were passed unanimously:—

"1. That a recent proposal from the Board of Guardians of the Poor of the Salford Union, to the Weekly Committee of the Salford and Pendleton Dispensary, renders it incumbent on the medical officers to consider the question, Whether the fact of their having hitherto attended and given medical aid to a large proportion of paupers gratuitously, has been consistent with the real interests of the medical profession?

"2. That as the Board of Guardians are empowered, by a clause in the Poor-Law Amendment Act, to provide sufficient medical and surgical relief for the poor under their protection, it appears evident that gratuitous aid is unnecessary, and actually injurious to such members of the profession as might be glad to avail themselves of that remuneration which the Board are called on to make for the required medical aid.

"3. That the circumstance of the clergy and gentlemen of the law being both paid for their respective professional services, is an additional reason why the same should be expected by the medical profession.

"4. That it is important to be remembered that the Manchester Royal Infirmary and Dispensary, and this institution, have been in the habit of affording medical and surgical aid to the entire number of applicants recommended by subscribers, without an inquiry having been made whether any were receiving parochial relief; and that the overseers of the poor of Manchester, Salford, and other townships, have been subscribers to the institution, and have availed themselves of the opportunity of obtaining medical relief for their poor, without any expenditure of the public money on *medical men*, excepting what has been paid to the house-surgeon of the workhouse, who,

it is believed, had very little duty to perform, in comparison with that given by the medical officers of the two institutions.

"5. That if the profession had done wrong in countenancing the admission of paupers into infirmaries and dispensaries, in the manner described, it seems, now that the New Poor-Law is coming into action, a favourable time to submit to the subscribers of such institutions their views on the subject, and to suggest the reasonableness and propriety of confining their aid to *independent labourers*, on the principle that there is a legal provision for medical relief for the paupers, and which also provides an adequate remuneration to medical men.

"6. That in order to ascertain whether practices, as described in the 4th resolution, have been allowed in infirmaries and dispensaries in Liverpool, Birmingham, Bristol, Sheffield, Leeds, Hull, Nottingham, Halifax, Leicester, Stafford, and Newcastle-on-Tyne, letters be addressed by the Chairman of this Board to the medical gentleman being at the head of a medical charity in each of the above places, requesting that he will call a meeting of his colleagues (physicians and surgeons) to consider and report on the question, and also to report to this Board their statements on the subject, as it is embodied in the foregoing resolutions, a copy of which should accompany each letter.

"7. That a letter be addressed by the Chairman (accompanied by a copy of the resolutions adopted) to the senior physician of the Manchester Royal Infirmary and Dispensary, requesting the opinion of his colleagues (physicians and surgeons) on the subject, and which opinion this Board will be glad, if they may be allowed to consider, will be the groundwork of a report to the Trustees of the Infirmary, in the event of a similar application being made by the Board of Guardians of the *expected* Manchester Union to that charity.

"8. That this Board can distinctly disclaim any intention on their part to become paid medical officers of the Board of Guardians of the Salford Union, and cannot, therefore, be considered as acting on the present occasion in any other way than with a sincere desire to benefit the best interests of the profession at large, and the private advantage of their junior professional neighbours.

(Signed) "GEO. GARDOM, Chairman."

This letter was laid before a special meeting of the Nottingham Branch of the BRITISH MEDICAL ASSOCIATION, when it was unanimously resolved :—

"1. That the contracting to furnish medical relief to parish paupers, by medical charities, is a perversion of their objects, degrading and unjust to the medical profession, injurious to the public, as tending to diminish amongst the poor that honest pride and love of independence which are the professed objects of the New Poor LAW.

"2. That this Association having learned that an offer of such a nature has been made to the Salford and Pendleton Royal Dispensary, by the Guardians of the Salford Union, think it incumbent on them to express, in the strongest terms, their disapprobation of such a proposal, as being both unjust and insulting to the medical profession.

"3. That the thanks of this Branch of the British Medical Association be given to Mr. G. Gardom and the Medical Board of the Salford and Pendleton Dispensary, for their promptness in bringing this subject under the notice of the medical profession.

"4. That the thanks of this Association be also offered to the medical officers of the Nottingham Hospital for laying the letter from the Salford and Pendleton Dispensary before the Society.

"5. That an account of these proceedings be forthwith forwarded to the Secretary of the Parent Association, and to the Editor of THE LANCET.

"J. M. DAVIDSON, Chairman.
"H. C. ATTENBURROW, Hon. Sec.
"Nottingham, Dec. 17, 1838."

DARK SIDE OF THE LIFE OF THE STUDENT OF MEDICINE.

FROM a "Lecture, introductory to the Business of the Original School of Medicine, Peter-street, Dublin," delivered by Mr. G. T. HAYDEN, the Lecturer on Anatomy and Surgery, we extract the following remarks on the "dark side" of the life of the medical student, a side which certainly should be well contemplated by every youth before deciding on devoting himself to the study of the healing art. Nor less attentive to these considerations should be the parents and guardians of young men whom they propose to apprentice to a profession which has, when fully understood, probably the fewest of all temptations to young men who are engaged in the active duties of life as a means of future subsistence :—

All professions, when well worked, are arduous, the medical calling pre-eminently so, both in its acquisition and its practice.

Believe me, the road to the Temple of Medicine is a series of rough and rugged ascents, truly an up-hill course,—a long and weary way, beset with mists, meteors, and mischances. It is true, if even schooled for the profession, the preparatory course, the classic lore, the mind-expanding science, in short, the liberal education, are all ways

of pleasantness: but, these open upon the narrow passage to the dissecting-room, for *there* you are to lay the foundation of medical knowledge.

Do not start, my young friend, at the sight of that pale and wan student cultivating anatomy; his hands besmeared with the blood and putrescence of the remains of decayed and disgusting mortality.

You go next to an hospital; look at those foul and fœtid ulcers which it is the student's duty to dress. Behold the surgeon cutting through the abdominal walls, in order to divide a band which has caused incarceration, inflammation, and mortification of the intestine.

You visit another ward: what case is that which excites so much anxious attention?—You approach the bedside of the patient; you gaze with surprise; for in his sadly altered features you recognise the countenance of him, once affluent and respectable, the sharer of your father's hospitality and friendship; so often the welcomed inventor of your youthful sports: alas! how changed the scene,—how altered the actor. The last fatal epidemic bereaved him of his beloved wife and darling children—in one short week all, held dear, removed. Melancholy marks him for her own. At length, he is persuaded by injudicious and false friends to attempt to dissipate his grief by plunging into the vortex of all the gay fashionable follies of the day; nay more, he is ultimately induced to drown his sorrows in the bowl. Oh! delusive and fatal remedy! The livery of melancholy was soon exchanged for that of revelry and drunkenness. Poverty and degradation—the usual attendants—quickly supervened; he has now become the confirmed drivelling drunkard, alike despicable and detestable.

But a few nights ago, returning from the public-house with unsteady pace, he staggered, fell, and in attempting to regain his feet, converted a simple fracture of the leg and dislocation of the foot into a most dangerous form of injury. He is now, unfortunate and ill-fated man, labouring under delirium tremens commonly called "whiskey fever."

Gentlemen,—believe me, there are heart-rending scenes of harrowing horror, presented in the practice of our profession, which language cannot pourtray. I need but mention that we too often witness the anguish of soul, the inexpressible grief, the all-absorbing sorrow, consequent upon the removal of the mother's only child.

The result of statistical tables of the length of life of the members of each profession, indicates that medical pursuits and practice tend to shorten the natural period of existence more than the duties of any other professional calling. How can it be otherwise?

Look at the labours, the studies, the incessant application, the fatiguing, the harrassing watchings, the exposure to night air, and to contagious diseases, when roused at midnight from a bed of weariness to visit a bed of fever; all these fully prove that the pursuit, the acquisition, the practice, of the medical profession are arduous in the extreme.

But, there is a portion of the dark side of the picture still undisclosed. You have viewed the student in the dissecting-room, surrounded by objects loathsome in the extreme, you have followed him to the hospital, and have witnessed the heart-rending scenes of suffering to which human nature is heir, both there, and in the everyday walk of life. Next, you see him in the study, wasting his health over the midnight lamp; or undertaking his labours long before the sun has risen to dispel the darkness and gloom of the night; lastly, you see him, pale, wan, and anxious, presenting himself as a candidate for a degree or diploma which is to indicate to the public his efficiency to practise and teach his profession. The result of a close, scrutinising, and searching examination proves that the student may undertake the arduous duties of the practitioner or teacher with safety and advantage to the public.

Hitherto, his way has been rough and rugged—he has eaten his bread in the sweat of his brain, as well as the sweat of his brow,—notwithstanding the struggle has been a triumphant one, for it has been crowned with success.

It is true, he has been licensed to practise,—he is naturally eager to exercise the functions of a practitioner, ready for the full fruition of the rewards of his labours, his long-continued and patient endurance of hardships and privations.

The overstocked profession; the avennes to public patronage, favour and support, filled, and choked up with anxious expectants; every standing-place pre-occupied and firmly maintained: there are difficulties, and obstacles, in our present times, to be encountered in every calling in common with medicine.

But, Gentlemen, it is as lamentable as it is true, that envy, hatred, malice, and all uncharitableness, which exist to a deplorable extent in the world at large, appear to have taken up their abode amongst some of the members of the medical profession,—a profession in which we should naturally expect to find quite the reverse, one ennobled and exalted by its very office—the God-like function of removing, or alleviating disease, preserving health, and prolonging life.

Strange, you will say, that men who have obtained a liberal education, remarkable alike for talents and acquirements, should take such narrow, unchristian, and uncharitable views. If this had arisen solely from

the arbitrary and unfounded distinction between physician and surgeon, that very distinction, although nominal, would still afford some grounds for difference and dispute. But far otherwise, the members of the same body, the several bodies themselves, taken collectively, are influenced by jealousies, motives, and objects unworthy of a liberal profession, and calculated to depreciate and degrade the whole in public estimation.

THE LANCET.

London, Saturday, December 29, 1838.

The facts which have been communicated to us by correspondents, from various parts of the country, and others which have been elicited at the Medico-Chirurgical Society, seem to demonstrate a frightful increase in the number of cases of small-pox during the last few years. From the first of January, 1838, up to the present time, six hundred and eighty-one patients have been admitted into the Small-pox Hospital, being an increase of thirty-five above the greatest number annually received into that establishment, even before the discovery of vaccination. Such a circumstance presents matter for grave reflection, and imperatively demands the discussion of certain questions, in which are equally involved the interests of science and of the public health. To what are we to attribute the increasing prevalence of small-pox? Has the number of cases of secondary small-pox latterly increased to a remarkable extent? Have we any reason to believe that the protecting influence of the vaccine virus is temporary? Or does it extend throughout the whole period of the life of the individual? Finally, how far do facts seem to indicate the necessity or the inexpediency of revaccination?

These are questions of the highest importance to society, and, before undertaking their discussion, we feel bound to declare that we have taken up the subject without any bias or prejudice. The elucidation of truth is our sole object. The facts which we are about to relate have been collected indiscriminately—impartially—from the scientific records of the different nations of Europe; and, if the opinions which we are about to express should be found to militate against hitherto-received doctrines, we shall be ready to alter our views whenever a more extensive and equally authentic series of facts shall be adduced in opposition to them. It is necessary, however, at the outset of this inquiry, to observe that neither individual opinions nor an inconsiderable number of facts can throw much light upon, much less determine, the various questions connected with the occurrence of secondary small-pox, the protective influence of vaccination, and the expediency of revaccination. This remark is addressed to several of our correspondents, and other scientific individuals, for whose acquirements we entertain the highest respect. Personal experience may, doubtless, justify the confidence which many respectable practitioners continue to place in the protecting powers of vaccination. A variety of isolated facts can frequently be brought forward either for or against any question in medical science; but, we again repeat, in order to avoid all sources of error in the important subject of our present discussion, individual opinions must be disregarded, and isolated facts must be thrown into the general scale, by which means alone their weight and relative value can be estimated.

The benefits which have resulted from the discovery of vaccination are too well established by the test of experience, and the concurrent testimony of mankind, to admit of a doubt; but a full acknowledgment of such advantages should not prevent us from investigating, thoroughly, the different points upon which the value of vaccination, as a protection against small-pox, is based. More than forty years have now elapsed since Jenner's discovery. The vaccine virus generally employed in this country at the present day, and, indeed, in most parts of the civilised world, is a reproduction of that which was originally introduced by its discoverer; in other terms, the virus has not

been renewed, except in a very few instances, from its original source, the cow. Hence, a natural question, which has frequently been discussed, arises, Does the vaccine virus of the present day possess the same protecting influence as that employed by JENNER, and his more immediate successors?

We are not prepared to answer this question, either in the affirmative or the negative, but can demonstrate the following proposition, which may throw much light upon it, viz., "That small-pox, which had evidently "been almost subdued during the first "twelve or fourteen years after JENNER'S "discovery, has latterly increased to an "extraordinary extent;" and, furthermore, "That the number of cases of small-"pox after vaccination is gradually in-"creasing as we recede from the period of "the introduction of vaccination."

The modifying effects of vaccination at an early period after JENNER'S discovery, and the increased prevalence of small-pox at a later period (an increase which cannot be accounted for by augmented population), are illustrated by the following facts:—

In Copenhagen, for the first five years after the introduction of vaccination, and, consequently, before the system was fully adopted, the annual mortality from small-pox was three hundred. During the next eight years, the annual mortality of the disease was reduced to sixty, but since then, as will be presently seen, the small-pox has again prevailed with great severity.

The records of Sweden furnish the following results:—

The deaths from small-pox were from—

1782 to 1791	47,587
1792 to 1801	44,184
1802 to 1811	14,904
1812 to 1821	3,309

It is impossible to demonstrate in a clearer manner than does the above table the happy results of vaccination for the first twenty years after its introduction into the Swedish dominions. Latterly, however, small-pox has been on the increase. In the year 1823 it spread widely over the whole untry, and in 1824 destroyed five hundred and sixty persons, of whom not less than one hundred and three, or nearly *one-fifth*, had been previously vaccinated.

The Reports from the Small-Pox Hospital of London show, in a still clearer manner, the gradually increasing prevalence of the disease. During the 25 years which preceded the discovery of vaccination, the average annual number of patients received was 286; from 1800 to 1824 the annual number was 143; in 1825 the number increased to 419; from 1826 to 1837 it averaged 270; from 1837 to 1838 the number amounted to 740. Hence, during the twenty-three years which followed JENNER'S discovery, the number of small-pox patients diminished by one-half. In the year 1825 this number became suddenly tripled. From 1826 to 1837 the annual number of patients was double what it was during the early period of the introduction of vaccination. From 1837 to 1838 the number of patients was five-fold; and, according to the recent statements of Dr. GREGORY, we may conclude that the number of persons affected with small-pox is daily on the increase.

Although JENNER himself, however prepossessed we may suppose him to have been in favour of his discovery, was fully aware that vaccination did not protect the individual always, and for the whole period of life, from the contagion of small-pox, yet many of his contemporaries and successors, with more zeal than discrimination, obstinately supported the affirmative. Experience has now determined the question beyond a shadow of doubt. Persons previously affected by small-pox may contract the disease a second time; in like manner, individuals, at any period after vaccination, are liable to be affected by small-pox, and do in many cases actually fall victims to the disease. Again; it has been observed that the small-pox virus gradually loses a portion of its intensity and virulence by successive transmissions through several individuals. These circumstances were sufficient to justify a suspicion that the vaccine virus might, by successive transmissions and the lapse

of time, be deprived of some part of its original protective power. Opinions were variously divided upon the point, but each party retained its own, because sufficient statistical data, whereon any decisive notions might be founded, did not exist. We have consulted a vast number of documents upon this question, and the result has been an *impression*, not amounting to *conviction*, though little removed from it, "That the "number of cases of secondary small-pox, "that is, the number of persons affected by "small-pox after vaccination, has gradually "increased in proportion to the lapse of time "since JENNER's discovery."

Formerly the recurrence of small-pox was considered to be an extremely rare phenomenon. LACONDAMINE estimated it at one in 50,000; HEBERDEN, at one in 10,000; other English writers of the older school, at one in 8,000. The occurrence of small-pox after vaccination has, also, been held to be as rare as that of small-pox after natural small-pox; yet, in one of the Copenhagen epidemics, MÖHL found that one in six of the persons attacked was affected with secondary small-pox; while in the Swedish epidemic of 1824, one-fifth of the deaths from small-pox occurred in individuals who had been vaccinated.

The history of the opinions successively put forth by the Committee of the National Vaccine Establishment illustrates, in a very curious manner, the proposition which we have just announced.

In the year 1809, Mr. BROWN published a work in which were detailed several facts calculated to shake the then existing belief in the efficacy of vaccination. In the year 1810, apparently with the object of neutralising any effect which might be produced by Mr. BROWN's publication, the National Committee announced in their Report,—"That since the establishment of the insti-"tution, *no person* vaccinated by them had "contracted small-pox, and that the cases "related by other physicians had been ad-"mitted without sufficient proof."

In 1811, however, the Committee was forced to modify this opinion, and admit "that such cases *might* exist."

In 1813 the Committee declared that cases of small-pox, after vaccination, were so extremely rare as not to deserve notice; and in 1814 they did not hesitate to announce, as "the unanimous opinion of prac-"titioners, that no authentic case of small-"pox, after complete vaccination, had ever "occurred."

In the year 1816 certain events in provincial towns compelled the National Committee to admit the existence of small-pox after vaccination, to account for which they invented some unintelligible theory of "an imperfect vesicle."

During the next four years we find, by an examination of the reports, a gradual change in the opinions of the Committee; and in 1820 they frankly acknowledged the impossibility of any longer adhering to the doctrines which they had held for a period of ten or twelve years. This change was evidently produced by the increased number of cases of secondary small-pox which occurred during the epidemics of 1818, 1819, and 1820.

Thus, we find that cases of small-pox after vaccination were extremely rare during the period which immediately succeeded Dr. JENNER's discovery, while they became so numerous towards 1820 as to overcome the obstinacy and prejudice of even the Committee of the National Vaccine Establishment.

The records of the Small-Pox Hospital furnish evidence of a similar tendency. In a table published by Dr. GREGORY, in the year 1823, the following proportions of persons attacked by small-pox after vaccination are indicated:—In 1809, one in 36; 1810, one in 30; 1811, one in 15; 1814, one in 20; 1815, one in 17; in 1818, one in 6; in 1819, one in 6; 1820, one in 6; 1821, one in 4; 1822 one in 3½; finally, in the present year, the proportion has increased to one in 2¼. Here, then, we discover that the number of vaccinated persons admitted with small-pox during the first five years, was as

one to twenty-two; while, during the last five, the proportion had increased to one in four and a half, and is now represented by two in five.

It is not in England alone that we find this remarkable increase of late years in the number of persons attacked by secondary small-pox. We have abundant evidence to show that similar occurrences have taken place in various other parts of Europe. Some cause, therefore, must operate in the simultaneous production of these analogous events, and that cause must be, either an increased virulence in the small-pox virus, or a decrease in the protecting power of vaccine. One correspondent, indeed, has suggested that as the population of this and other countries gradually increases, so we may naturally expect a gradual increase in the number of small-pox patients. This argument is just to a certain extent; it applies to all diseases indiscriminately, but it cannot account for the differences which are exhibited in the above statistical records. Who, for example, could venture to affirm that the population of the Metropolis was doubled, tripled, or quintupled within the space of a few years? A somewhat similar argument has been advanced in a contemporary journal, and on applying it we arrive at the astounding fact, that in the year 1832 the state of Denmark contained sixty nine times more vaccinated persons than it did inhabitants!

The records of Denmark demonstrate also the gradual increase of cases of secondary small-pox, as we recede from the period at which vaccination was introduced into that kingdom. From 1800 to 1804, *not a single* example of small-pox after vaccination was observed at Copenhagen.

During the year 1804 *two* cases, of a mild form, were observed.

In 1805 *five* persons died of secondary small-pox; and in 1806 three individuals died of a similar disease.

In 1824 no less than 257 cases of small-pox after vaccination presented themselves; in 1824 and 5 the numbers amounted to 315; in 1826 and 7 to 438; from 1832 to 1834, out of 1015 cases of small-pox, 898 occurred in vaccinated persons; and in the year 1835 there were observed 1043 cases of secondary small-pox.

The documents furnished by France are few in number, because the statistical returns have been made, for a series of years, to the Royal Academy of Medicine, in whose archives they are allowed to remain as waste paper. We may, however, cite one or two illustrative examples. An epidemic of small-pox prevailed at Milhau in 1817; even then the possible occurrence of the disease after vaccination was doubted. The corporation of the town ordered an inquiry to be made, and it was discovered that 120 vaccinated persons had been attacked by small-pox.

At the close of the epidemic at Marseilles, in 1828, the General Council of the Department likewise instituted an inquiry upon this point, the result of which showed that of 864 persons attacked within the space of six months 478 had been vaccinated.

The laborious and apparently accurate publication of Dr. Heim, on the variolous epidemic which prevailed for five years (1831 to 1836), in the kingdom of Würtemburg, furnishes numerous illustrations of the proposition which we have just discussed. The number of vaccinated persons attacked by small-pox in Germany, as in other countries, has greatly increased within the last few years. It is unnecessary to enter into statistical details for the purpose of proving that during a considerable lapse of time after the introduction of vaccination, the occurrence of secondary small-pox was excessively rare. This is admitted on all hands. But what do we learn from the Würtemburg epidemic? Let facts answer. The number of well ascertained cases of secondary small-pox, during the epidemic, amounted to 917, and if we include those who presented imperfect cicatrices, to 1055. The total number of persons attacked amounted to 1677. The accuracy of this statement may, we believe, be relied on with confidence. The kingdom of Würtem-

burg is divided into a great number of districts, over each of which is placed a medical man, specially charged with the duty of vaccination, &c., and the reports which have been forwarded from the respective districts abundantly testify the zeal and ability which were displayed in the discharge of the duty entrusted to these officers.

In the different districts, facts of an equally remarkable nature presented themselves. We select a few from amongst a multitude. During the epidemic at Tuttlingen, in 1833, Dr. Roesch had charge of 59 cases of small-pox, and of these 52 occurred in vaccinated persons. At the same period, in the neighbouring district of Schwenningen, 78 cases were observed, nearly all in persons who had been previously vaccinated. Not one case occurred in children *below* seven years of age. In these the protecting influence of the vaccine matter acted with its full force.

It remains to say a few words on the facts furnished by Austria and Italy, for which we are indebted to the learned Editor of the "Experience." Dr. Verson, district surgeon at Parenzo, in Styria, informs us, that from 1802 to 1819 cases of secondary small-pox were so rare in his district, that few persons believed in the possibility of their occurrence. The number, however, gradually increased until 1825, when the cases became sufficiently numerous to remove all doubts,—even to excite the most uneasy apprehensions. The number of cases of variola in Parenzo, during 1832-3, amounted to 300, in the *majority* of which there had been previous vaccination.

In 1826 Moro observed 72 cases of secondary small-pox in 137 patients.

In 1831 Gambriani assures us that, of 748 individuals attacked by various degrees of variola, 643 had been vaccinated.

We have thus adduced a pretty extensive mass of statistical evidence which seems to us to establish the proposition, "That se-"condary small-pox has become infinitely "more frequent than it was during the first "ten or twelve years after the discovery of "vaccination." Should contradictory evidence present itself, or should any facts of a different tendency come within our knowledge, we need scarcely say that we shall lose no time in laying both before our readers.

In our next article we propose to examine how far the protecting influence of vaccination is modified by the period of time which may have elapsed since the introduction of the vaccine virus into the system of the individual.

PERSHORE UNION.

CASE OF THE UNFORTUNATE THOMAS ATTWOOD.

In consequence of a letter which we received a few days since from a Member of the Board of Guardians of the Pershore Union, requiring that we should give up the name of the correspondent who sent to The Lancet (page 311, present Vol.) the report of the alleged treatment of poor Attwood, a pauper, denying, *in toto*, the truth of the statements, we immediately addressed a note to the gentleman from whom we had received the account, Mr. Francis Davies, surgeon, of Pershore, asking if he objected to the announcement of his name, and whether he could furnish any collateral testimony relative to the accuracy of his report. The following note and documents exhibit the straightforward and fearless replies which Mr. Davies has sent in answer to our questions:—

To the Editor of The Lancet.

"Tiens à la verité."

Pershore, 16 Dec., 1838.

Sir:—I have to acknowledge the receipt of your letter, requesting me to investigate the case of Thos. Attwood; and in answer beg to state, that it is my firm opinion that the report in The Lancet is substantially true, and I am fully borne out in that opinion by the inclosed correspondence. I beg to add, that I called on Mr. Sheppard, under whose care the very unfortunate man was when in the infirmary. Mr. Sheppard *positively refused* to say one syllable on the subject. I *afterwards* wrote to the house-surgeon, Mr. Cole, as also to Mr. Pierpoint, and Mr. Martin. Their letters I have sent, so you will be able to judge whether the

statement in THE LANCET is "wholly false" and "perfectly untrue."* I remain, Sir, your obedient servant,

FRANCIS DAVIES.

Mr. Davies to Mr. Cole.

"Pershore, Dec. 15, 1838.

"My dear Sir:—Will you do me the favour of saying whether the statement made in THE LANCET of Nov. 17th is substantially correct, as far as regards the Worcester Infirmary. I remain, dear Sir, yours very truly,

"FRANCIS DAVIES.

"To Herbert Cole, Esq., &c., &c.
Worcester Infirmary."

Mr. Cole to Mr. Davies.

"Worcester Infirmary,
"Dec. 15, 1838.

"My dear Sir:—I have seen the case of Thos. Attwood, as reported in THE LANCET of the 17th of Nov., and it is correct as far as the infirmary is concerned, with the following exceptions: the hernia had been down, from the man's own account, six-and-thirty hours;† the integuments were black, but not sphacelated. I remain, my dear Sir, faithfully yours,

"HERBERT COLE.

"To F. Davies, Esq."

Mr. Pierpoint to Mr. Davies.

"Worcester, Dec. 15, 1838.

"My dear Sir:—I have seen the statement in THE LANCET of Nov. 17th, respecting the case of Thos. Attwood. The report of the poor man's state is generally correct, as to the description of him when brought to the infirmary. The bowel had been strangulated much longer than the report mentions; in other respects I see no reason why the report of his case and consequent sufferings should be found fault with. The poor man was under the immediate superintendance of my colleague, Mr. Sheppard. Yours faithfully,

"J. PIERPOINT.

"To F. Davies, Esq., Pershore."

Mr. Martin to Mr. Davies.

"Pershore, Dec. 16, 1838.

"Dear Sir:—I have read the statement in THE LANCET of Nov. 17th, respecting Thos. Attwood, and, as far as my name is concerned, it is perfectly true.

"JAMES MARTIN, Guardian."

* The expressions used by the Member of the Board of Guardians in denying the truth of the report.—ED. L.

† In the report in THE LANCET it was stated that the hernia had been down only *twenty-four* hours.—ED. L.

WESTMINSTER MEDICAL SOCIETY.

Saturday, December 22, 1838.

Mr. HALE THOMSON, President.

NEW BED FOR INVALIDS.—HEART DISEASES.

A VERY ingenious bed, invented by Mr. KNOX, of Jermyn-street, was exhibited to the Society. The bed is constructed somewhat upon the principle of the celebrated fracture-bed of the late Mr. Earle, but possesses many and very great advantages over that machine; the patient can, without the slightest inconvenience, be placed in any position that may be required, his linen be changed, and all the necessary evacuations effected without, in the slightest degree, disturbing him. It certainly possesses great advantages over every other invention of the kind. The bed excited great interest in the members, several of whom spoke in high terms of its usefulness, and considered it a boon to the profession.*

Mr. D. O. EDWARDS referred to three cases in which this apparatus had been of essential service in St. Thomas's Hospital. One of these cases was that of a man, thirty years of age, who fell and fractured the first lumbar vertebra, the injury being accompanied with displacement of the upper portion of the fractured bone; the sphincters were paralysed, and complete paraplegia existed. He was placed on Mr. Knox's bed on the fifth of July. As the urine and fæces passed involuntarily it was necessary to change his linen, blankets, and bedding very frequently; this was effected without occasioning the slightest inconvenience to the patient; the bed was often moved for the convenience of the sufferer by the nurses, the other patients, or the friends; the patient was placed occasionally on his back, or his side, or prone, according to his own pleasure; he lay prone for various lengths of time, such as periods of 26, 18, 12, 10, or 6 hours; he generally preferred laying on his back or stomach to laying on his side, on which he seldom remained longer than a couple of hours. Towards the end of his life he was incapable of laying in any position for more than two or three hours. In order to change his bedding the attendants moved the bed round till the face of the patient was downwards. Whenever he was fed an inclined plane was formed by moving the plane of the internal frame upon an axis, and thus raising the head and depressing the feet to any extent desired, and to relieve the ennui of uniform position. Whilst he was supine his lower limbs were placed on a single or double inclined plane.

* Mr. KNOX, in answer to a question, stated that the cost of the bed was seventeen guineas, which, considering the great quantity of work in it, is cheap.—REP. LANCET.

The patient lay on the couch from the fifth of July till the sixth of December, when he died. He always expressed himself grateful for the great ease and confidence of security which the apparatus afforded him. An examination after death exhibited the spinal marrow, though not lacerated, pressed upon by the displaced arch of the vertebra, but perfect ossific union had taken place between the broken surfaces which were kept in contact, a fact showing the perfect quiescence secured to the fractured parts by the apparatus. In the second case, the patient fractured one of the lower dorsal vertebræ. Mr. South trephined him, but, upon removing the arch of the vertebra, the spinal chord was discovered to be completely divided; of course no ultimate good occurred from the operation; the bed, however, was found of great use; the patient was raised from the operating table, and placed prone on the bed; the bedding was placed over him, and he was gradually, and, without succussion, turned round till he lay supine. In this way, during the short remainder of his life, he was moved, and his fæces removed and dressings altered without exciting any motion in the injured part. A third case is one of concussion now laid on the bed; the patient is recovering, and is ardent in the expression of gratitude for the comfort he has experienced.

Mr. Hale Thomson bore testimony to the value of the bed as employed in some cases in the Westminster Hospital. Other members also spoke in high terms of the apparatus, and much interest was excited by the demonstrations of the facility with which the different changes were effected, as shown by Mr. Knox.

OPEN STATE OF THE FORAMEN OVALE.—BLUENESS OF THE SKIN.—THE EXCITO-MOTORY SYSTEM.

Dr. Bird exhibited the heart of a patient, respecting whom he related the following particulars:—He was consulted on the 15th of November by a tall, well-formed, young woman, twenty-eight years of age, who presented an exquisite specimen of chlorosis, having all the symptoms of that condition marked in a high degree. She complained of unpleasant feelings about the uterine system, occasional palpitation of the heart, with dyspnœa on going up stairs, and on making exertion; she said she had suffered from the dyspnœa from her infancy. Purgatives, followed by the preparations of iron, relieved her. About a month after this Dr. Bird received a hasty summons to attend her, as it was expected she was dying. He found her lying on the bed suffering from extreme dyspnœa, and breathing with a loud crackling noise; her countenance was intensely anxious and pallid; she had not spit blood; there was evidence of excessive emphysema of both lungs in the anterior region of the chest; no sound indicating its presence, however, was detected behind. On examining the heart a loud, sharp sound, like the *bruit de rape*, was audible; at first it was taken little notice of, as occurring in connection with the chlorosis; the next day, however, on examining the cardiac region, the sound was peculiarly sharp and grating; it accompanied the first sound of the heart, and was heard loudest just to the left of the sternum; it was not distinct posteriorly, probably from the presence of the emphysema; the sound was pretty constant; it was more audible under excitement, and was not affected by change of position. On inquiring further into the history of the patient he was informed that when she was about seven years of age, after great exertion, she suffered from palpitation, and her countenance assumed a colour like that observed in persons who were cold; she had never suffered from the symptom since; she died suddenly under a little mental excitement. On examination after death the contents of the abdomen and pelvis were found healthy, with the exception of that condition of the ovaries which precedes dropsy; there were marks of extreme emphysema in the lungs; the heart was firmly contracted, and there was a fibrinous deposit in its left cavity; the left ventricle was slightly hypertrophied; the valves were healthy, with the exception of a slight roughness of one of the aortic valves; the foramen ovale was patulous, and would admit the point of the little finger, and was guarded by a valve, which, though it prevented the blood from escaping from the right side, admitted its entrance from the left; the mitral valve was a perfectly healthy specimen. The case was interesting, as showing the presence of an opening between the auricles, though there had been no blue tinge of the skin for twenty years. Cases of an analogous kind were recorded, but in this one he (Dr. Bird) considered it difficult to account for the bruit. Did it arise from the slight roughness of the aortic valve?

Dr. Williams said that it was difficult to account for the bruit; he did not consider it arose from the slight roughness of the aortic valves, as he had frequently observed their roughness in cases in which there was no murmur; it was not at all uncommon in patients of thirty or forty years of age. He was inclined to think the mitral valve was the seat of the sound, for though it might be, at the first glance, considered perfectly healthy, yet the slightest displacement of it would allow of the passage of blood. He had seen many cases, particularly in chlorotic females, in which this displacement was the only sign to account for the presence of the murmur. He believed that there was always something imperfect, either congenital or acquired, before mur-

murs could be produced, even in chlorosis. The condition of the blood in this affection, it was true, was such as to lead to the more easy development of these imperfections. He had seen many cases in which this defect consisted in a slight disease of the mitral valve. In the case under consideration there was rigidity of the entire surface of the valve, with a degree of thickening of its margin. The blue appearance of the skin was not always present in cases where the foramen ovale was open; but this symptom was very rarely absent if there was obstruction also to the pulmonary circulation, as this obstruction produced a more patulous condition of the foramen. In the case related by Dr. Bird, however, though there was pulmonic disease, and, no doubt, admixture of the venous and arterial blood, this blueness did not occur. What was the cause of this? He would inquire if there was really enough of blood in the face and lips to produce discolouration; he thought not. He had seen several cases of this change of colour arising from diseased heart; chlorotic patients did not show it well. It was necessary that the capillaries should be more distended with blood than is the case in chlorosis, for the discolouration to take place.

Dr. BIRD recollected a case in which there was an opening in the septum ventriculorum, and a patulous state of the foramen ovale, and the patient was never blue until a few days previous to death. He had thought the mitral valve healthy in the case he had first related, because on pouring water into the left ventricle it was retained by the valve.

A MEMBER considered that in cases where the foramen ovale was open, and there was no blueness, it did not necessarily follow that the occurrence of pulmonic disease should produce it; there might be obstruction to the flow of blood through the lungs, and yet not be greater pressure on the parietes of the heart. Cases had occurred, and Dr. Bird's case was one, in which the open foramen was valvular in form, and would resist the escape of blood from one auricle to the other, on whichever side the pressure was exerted. He did not consider that the state of the blood in chlorosis was such as to account for the absence of the blue tinge in the countenance as diagnostic of an open state of the foramen ovale, for if you pricked any part of the skin with a needle blood would flow.

Mr. GREGORY SMITH had examined a great number of hearts, and he had found in the majority of cases that the foramen ovale was open, though rarely to the extent observed in Dr. Bird's case.

Dr. WILLIAMS could not agree that the form of the foramen in the case under discussion, would induce a valvular action in both directions. Any undue pressure from the right side would, he believed, force the blood through the opening. Of course, if the lips of a chlorotic patient were pricked by a needle blood would flow, blood being necessarily present for the nutrition of the part. What he contended was this, that as there was not sufficient blood to produce a natural colour in the skin, the slight admixture of the arterial with the venous blood would not be observable. Dr. Williams then related a case in which a woman was free from any discolouration of the skin until her twenty-fifth year, when phthisis setting in she became much discoloured. In this case there was an opening through the foramen ovale, which was valvular and oblique in its direction, producing closure when pressure was exerted from the right, but none when the pressure was from the left. Regarding the test employed by Dr. Bird to prove the normal state of the mitral valve, he had long since abandoned it as worthless. It was no proof that the valve was sound when the passage of water was prevented, neither was the passage of water any proof that the valve was diseased.

Dr. BIRD said that Andral had found the number of cases in which the foramen ovale was open to be about one-half per cent.

Some conversation followed on various physiological points, which offered little for report, except the following remarks by Dr. Hall, and a statement made by Mr. Horne, which caused some astonishment in the meeting, to the effect that in several cases of cut-throat in suicide which he had seen, where the parts were divided even to the spinal marrow, priapism always existed.

Dr. MARSHALL HALL said that his views of the nervous system, so much called in question, might be conveyed in a mere detail of a series of indubitable experiments, without one word of commentary, the inferences from them flowing so naturally as to occur to every mind. With this object Dr. Hall exhibited drawings of the turtle, and enumerated the experiments made by him on that animal, with which our readers are already well acquainted. These experiments (said Dr. Hall) were not only new in themselves, but unfolded entirely new modes of action in these parts of the nervous system,—modes of action at variance with the laws previously laid down by Haller and Prof. Müller. If the trifacial nerve be irritated in its distribution on the nostril, or the palatine fringes, or the pneumogastric, in its distribution to the larynx, or even the trunk of the pneumogastric be divided in the neck, an immediate act of inspiration is induced. Is not this experiment an application of the *vis motoria* to physiology, the first ever made? Does it not display the anatomy and the physiology of a peculiar set of nerves,—nerves pursuing their course, not *from*, but *into*, the spinal marrow,—and inducing, under the influence of a stimulus,

appropriate actions? In a word, does it not set forth, in the clearest manner, the fact of the existence of appropriate excitors of inspiration? In his former remarks, Dr. Hall showed, by a detail of experiments, equally plain, that the trifacial nerve is the excitor, the facial the motor, in the action of the orbicularis in the closure of the eye. It has recently been shown, by Dr. J. Reid, that the superior laryngeal is the excitor, the inferior the motor, in the closure of the glottis. The action of the pharynx, œsophagus, and cardia, in deglutition; of the expulsors of the fæces, and of the semen, and of the sphincter, depends upon similar reflex actions through excitor and motor nerves, and their universal medium, the spinal marrow. Is it not, then, proved that the vis motoria acts in a direction not known to former physiologists, and that this action takes place through special nerves, and that, therefore, there is a distinct system of such nerves; that this action is the ruling principle in the important acts of inspiration and deglutition, and, in a word, in all ingestion and egestion; not one of which things could be imagined or understood without the first experiment which he (Dr. H.) had detailed—the very first step in this demonstration, for so he would venture to call this plain and simple detail. If he be asked whether there be distinct sentient and excitor, distinct voluntary and motor fibres, he would reply that he thought there were. Both experiment and pathology render this most probable, to say nothing of Mr. Grainger's interesting investigations, on which he (Dr. Hall) was not prepared to offer an opinion; but that such a view forms no part of what he conceived to be demonstrated. He had thus shown them:—1. That the vis motoria acts in directions not known before. 2. That it acts along incident excitor, or incident motor nerves, not known as such before. 3. That it has an exclusive application to physiology, not known before; he did not mean, to refer to the "sympathetic actions," but the actions of ingestion and of egestion, and of the orifices and sphincters, &c. &c. He had here stated what was positive in this matter, and confidently left the result with the Society, happy, indeed, if he had excited in its members any interest in this investigation. They had now only to read the various works on physiology, upon the acts of deglutition, of inspiration, &c. &c., to discover in what an inextricable maze of contradiction and ignorance we were, six years ago, upon this extensive series of functions.

CONDITION OF THE IRISH.

DR. MAUNSELL'S LECTURE.

To the Editor of THE LANCET.

SIR:—In your Journal of Saturday last (Dec. 15) I perceive there is an extract from a letter of a correspondent of yours, who is displeased at my having made the following assertion:—"Take the case of an infectious fever breaking out in one of those small villages into which the Irish peasants invariably congregate their dwellings. A member of one family is attacked; he, such is the utter barbarism of the country, sleeps under the same blanket (not even protected by night-clothes) with his father, mother, sisters, brothers, sisters-in-law and brothers-in-law."

Your correspondent denies the accuracy of this statement, on the grounds,—1st, That he never saw such a case; and 2nd, That the Irish are too religious a people to admit of such events occurring. In answer to your correspondent's first position, I can only say that there may be many things in this world which he never saw; but that I saw the case which I described, with my own eyes, many hundred times, and that any one who ever examined the interior of an Irish cabin of the lower class, needs no proof to convince him that all its inhabitants must occupy one bed and be covered by one blanket, if, indeed, they possess so great a luxury. With regard to "J. T. H.'s" second position, it is the first time that I have heard that religion furnished men and women with night-shirts and night-shifts. I should not have noticed this matter, but that you have, no doubt inadvertently, permitted my observations to be termed, in your list of contents, "Aspersions on the Irish."* I certainly consider poverty and misery to be no disgrace, and I have yet to learn that it is an "aspersion on the Irish" to state that they are poor, wretched, and neglected. If your correspondent supposes that I meant to cast any imputation upon their morals he does me an injustice. I know them well, and have had as much opportunity of closely observing them as most men of my age, and I have no hesitation in stating my belief, that in the particular to which "J. T. H." alludes they are as moral a peasantry as any in Europe. I wish, from my heart, it were also in my power to say they are as

* The word *alleged* was accidentally omitted from before the term "aspersions." However melancholy were the facts recorded by Dr. Maunsell, he could not have mentioned them with a view to reflect on the character of his countrymen. The condition of the indigent and suffering portion of the community in Ireland is the result of misrule, not of choice or disposition.—ED. L.

well clad, fed, and lodged. I shall feel much obliged by your giving insertion to this note, and am your obedient servant,

H. MAUNSELL.

Moleworth-street, Dublin,
Dec. 18, 1838.

DOSES OF MORPHIA.
ANALYTICAL CHEMISTRY.

To the Editor of THE LANCET.

SIR:—If you find, on perusal, the following to merit a place in your excellent Journal, your giving insertion to it will confer an obligation on one of your earliest subscribers. Your Number of THE LANCET for Nov. 10th contains an account of a supposed death from an overdose of morphia. Having been in the habit of prescribing morphia in large doses, in some cases to the extent of four and six grains, I was naturally much surprised at the statement in the article referred to, and sent that Number of THE LANCET to Mr. S. Burgess, of 20, New-street, Dorset-square, who has repeatedly dispensed my prescriptions for the large doses stated above, requesting his opinion on the subject. The following is his reply:—

"Sir:—With a view of ascertaining the accuracy of Messrs. Battley and Heathfield's statement in THE LANCET you were kind enough to forward to me, concerning the duplicate of a draught supposed to have caused the death of a lady at Dorchester, I made three separate portions of camphor mixture with *distilled*, *river*, and *fresh spring water*, and in two drachms of each I dissolved half a grain of acetate of morphia, weighed very exactly. I evaporated each separately to dryness, and the following is the result:—That made with distilled water yielded a residue of 0.45 gr.; that with river water 0.5 gr., and that with spring water 1.9 gr., this you will perceive is as much as 2 grains. Each, of course, answered to the test of morphia, and on further examination of the last-named, I discovered the presence of muriate of soda and sulphate of lime in sufficient quantities to account satisfactorily for the surprising difference in the weight. It may be as well to add, that it is a very common practice to prepare the camphor mixture with spring water, on account of its dissolving a larger portion of camphor acid; I am inclined to believe that Mr. Arden had done so, and that had Mr. Battley done as I have, by the the draughts submitted to him, his statement would have worn a very a different aspect. I am, Sir, your obedient servant,

"ALFRED BURGESS,
"Operative Chemist.

"To Dr. Edward Blundell."

It is surprising on what trifles the reputation of a medical man often stands, and I should consider it an injustice to Mr. Arden to let the article in THE LANCET on the subject remain unanswered; the impression conveyed in it is, certainly, that a life was sacrificed to his carelessness.* Why was there not a post-mortem examination? Mr. Arden's character is, with me, perfectly unimpaired. I feel convinced that had an examination of the body taken place, the death might have been accounted for from other causes, unless, indeed, the half grain of morphia has produced the lamented result in a very singular case of idiosyncrasy, which, of course, no human foresight could have prevented. I remain, Sir, yours faithfully,

EDWARD S. BLUNDELL.

18, Lower Seymour-street,
Dec. 18, 1838.

LETTER FROM MR. J. H. CURTIS.

To the Editor of THE LANCET.

SIR:—In reference to a letter signed "J. T.," which appeared in the last week's LANCET, I beg to say, that I have long since found, by experience, that I am much better occupied in attending to the duties of my profession than in replying to the objections of an anonymous writer. The question between us is, of course, altogether one of fact; and I repeat the substance of my former communication by saying, that in the large majority of cases of deafness which have come under my notice (which in twenty-two years' experience have not been a few), absence of cerumen has been a marked symptom; and that restoration of the healthy function of the ear has always been accompanied by increased secretion of cerumen. I have found, also, that the use of creosote has succeeded in producing this effect more rapidly, and more certainly, than any other remedy; and on these two facts I founded my communication. That the secretion of wax is a natural function of the healthy ear, will, of course, be admitted by all; and as Nature never made an organ in vain, nor gave it a peculiar function that was useless in the animal economy, it appears to follow, as a necessary consequence, that either as effect or cause, the interruption of the function must be connected with some kind of disease.

The verification of my cases would leave this question resting still upon my assertion, to be confirmed or neutralised by the experience of others; and as, therefore, no good result could follow from a discussion which would occupy much of my time and atten-

* If Dr. Blundell will re-read the last few lines of the article alluded to, he will arrive, we venture to affirm, at an opposite conclusion.

tion, I must decline the invitation of your correspondent. I am, Sir, your obedient servant,

J. Harrison Curtis.

2, Soho-square, Dec. 12, 1838.

IMPERFECT AND UNNECESSARY INQUEST AT ISLINGTON.

To the Editor of The Lancet.

Sir:—Your valuable Journal has often taken up the subject of medical coroners, as the most fit persons to preside at inquests. I think the necessity was never more strikingly exemplified than at an inquest held in Islington, at the *Albion Tavern*, on Friday, the 21st inst., on the body of Mrs. Caseltine, who was found dead in her room. I have requested of Mr. Desormeaux, a copy of a letter which he furnished to the Coroner, previous to the opening of the court. He expresses in it his opinion that *a satisfactory verdict could only be obtained by a medico-legal investigation.* In the absence of it, in my humble opinion, the inquest was a mere mockery. It is as follows:—

"*To —— Stirling, Esq., Coroner.*

"Sir:—In the case of Mrs. Caseltine, of No. 18, Albion-grove, Thornhill-road, who was found dead in her room, I beg leave to state that on examining her I found a slight bruise on the left hip, with considerable ecchymosis about the left hypochondrium, and a groove round the neck, as if she had made an attempt to strangle herself. The depth of the groove, when I first saw her, was sufficient to lay the little finger in. The body had been moved and laid out before I was called in. The fæces had been discharged, and it appeared as if she had vomited considerably about the room, in various places. I have ascertained that she had been disordered in her mind for some years. The son was *the only person living with her.* Although he is living in the house, he says he has not seen her *for seven weeks* previous to this occurrence, and that by her death he shall come in for £1000. As it is impossible to ascertain the cause of her death without *anatomical examination*, and *analytical investigation* of the contents of the stomach, and the matter thrown off from it, I requested that the corpse and the room might remained untouched until you were acquainted with the particulars, lest the public might be dissatisfied with a mere superficial investigation. If you think it necessary that further medical examination should be attended to will you please to request the gentlemen of the Jury not to trample upon the spots in the room *where she has vomited* or thrown down her liquid food. The aunt informs me that when the mother and son were together they quarrelled incessantly, which is not much to be wondered at if what the aunt has stated is correct. Looking at *all the circumstances of the case*, I felt it my duty to request the parish officer to call on you, and also to make this communication. I am, Sir, respectfully yours,

"D. Desormeaux.

"3, Brunswick-terrace, Barnsbury-road."

I do not mean to impugn the uprightness of the Jury, who were respectable men, and decided according to *the opinion of the Coroner*, and *such* evidence as came before them, but that worthy officer appeared to me not only to be decrepid from age, but not quick to the points which ought to be attended to.

Had a medico-legal investigation taken place it is most likely that the same verdict would have been returned; but as in the absence of all proof by analysis, nothing could be discovered in what was vomited up, or by examination of the stomach and its contents, or in the many vessels of the room containing fluids, curdled milk, &c. &c., a verdict must ever remain unsatisfactory. I do not for a moment entertain suspicion against any one in this case, or believe that there has anything wrong been done; but if persons are found dead under the circumstances of Mrs. C., without the most prying inquiry into the truth, what a door is open to crime! Some of the Jury inquired if it was not intended to have the body opened, when the clerk, who took the notes and put the questions, said, if the Coroner thought it necessary, after examining the witnesses, he would give directions. Mr. Desormeaux stated openly *in the court*, that without inspection of the body, and chemical examination, he could not give an opinion as to the cause of death. The verdict of the Jury was,—"Died by the visitation of God!!"

I am, Sir, your obedient servant,

F. Morris.

3, Brunswick-terrace, Islington,
Dec. 24, 1838.

*** A remonstrance should be entered in all such cases (in the manner which we have several times described in this Journal) against the payment of any fee to the Coroner, for presiding at the "inquest." The public have the remedy in their own hands. —Ed. L.

LETTER FROM MR. SIMPSON.

To the Editor of The Lancet.

Sir:—The readers of your valuable Journal must now be well convinced, as I have been for some time, that Dr. Geo. Gregory is not only one of the most "distinguished," but, also, one of the most "amusing" writers of the day. He certainly excels in evading matters of fact. My first letter was a mere statement of events as they

occurred, to none of which, in his brilliant epistle, has he replied, but asserted, that I was the only medical man from "Hyde-park-corner to Mile-end," who had ever complained of want of civility at the Small-pox Hospital. Mr. Chatto's letter was evidence in support of my statements, and I preferred its publication to that of others, because Dr. Gregory must have known of Mr. Chatto's complaint at *the very time* that he was attempting to mislead the public in his reply to me. I shall leave your readers to judge whether Dr. Gregory has not himself confirmed my statement, and thus shown the propriety of *inquiry* upon the subject. If they think that the charge of rudeness and want of courtesy has been justified by Dr. Gregory's own letters, well and good; if not, he has pointed out where they can be well attended to, which is all that I wanted my medical brethren to know from the first.

If it be true that so many children were vaccinated last year by Dr. Gregory and Mr. Marson, I sincerely hope they used better lymph than they were in the habit of giving to medical men who applied for it then. I also hope, that Dr. Merriman and others, in putting searching questions to the Doctor occasionally at the Medico-Chirurgical Society, on the subject of vaccination, will insist upon his keeping to the point at issue.

I am confident that your medical readers will feel obliged for Dr. Gregory's manly statement of the rules and regulations of the place "over which he presides." Every person must admire the liberality of the principles therein avowed. I had hitherto understood that the subscribers wished every encouragement and assistance to be given to those who advocated and practised vaccination; but it appears, from Dr. Gregory's last letter, that I was mistaken, and that it is for the benefit of *him*, and his "reports" alone, that the place was instituted or exists. I am, Sir, yours most obediently,

W. Simpson.

111, Guildford-street, Dec. 24, 1838.

A DISPENSARY ADVERTISEMENT.

To the Editor of The Lancet.

Sir:—Allow me to call your attention to the following advertisement in the "Morning Chronicle" of the 19th of the month:—

"Cancer and Scrofula, &c., removed without the knife.—The dispensary is open daily for the reception of patients. Established in 1821, since which, upwards of 3250 afflicted poor have been cured of glandular complaints by a means more easy and effectual than the knife. Supported by voluntary contributions. For particulars apply to Sir Charles Aldis, Old Burlington-street, St. James's, the surgeon and founder of the institution, who may be consulted daily till 12, and from 3 to 5."

Such is the state of our profession, that the President of the College of Physicians, a body which holds itself to be the highest and most select in the three kingdoms, is not ashamed expressly to patronise, and to introduce at court, the author of this advertisement. Some have said that the Good Samaritan of the Scriptures was a physician, but we have now both this authority and the example of Sir Henry to the contrary. I am Sir,

A Constant Reader.

Degree of M.D.—(*From a Correspondent.*)—The Universities of Edinburgh, Glasgow, and St. Andrew's, and the Colleges of Aberdeen, require that candidates for their degree of M.D. shall have attended courses of six months duration, and consisting of not less than one hundred and ten lectures, on the following subjects:—Anatomy, chemistry, institutes of medicine, surgery, materia medica and pharmacy, practice of medicine, midwifery and diseases of women and infants.

The above number of lectures cannot be included in any course in which the lectures are not given *more than four times* in each week during six months.

BOOKS RECEIVED.

On Granular Degeneration of the Kidneys, and its Connection with Dropsy, Inflammation, and other Diseases. By Robert Christison, M.D. Black, Edinburgh, 1839. 8vo., pp, 288.

The Discovery of the Vital Principle, or Physiology of Man. Starling, London, 1838. 8vo., pp. 566.

Essays on the most Important Diseases of Women. By Robert Ferguson, M.D., &c. Part I. Puerperal Fever. Murray, London, 1839. 8vo., pp. 299.

Manual of Descriptive Anatomy, by J. F. Meckel. Translated from the French translation, by A. S. Doane and others. Henderson, London. 2 Vols. 8vo, pp. 1220.—[Although much disinclined to encourage second-hand translations, we cannot avoid recommending the present one of Meckel's Anatomy.]

An Improvement in the Pathology and Treatment of Small-pox. By Robert Stevens. Renshaw, London, 1838.

TO CORRESPONDENTS.

J. H. B. The case alluded to has already appeared in The Lancet.

The communications of *Messrs. Crowfoot, Marshall, Jeffreys*, and *Walker*, shall appear next week.

The paper on the *plague*, although evincing considerable research, is not suited for the pages of this Journal.